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EDOUARD CLAPARÈDE

1873–1940

The death of Professor Claparède removes another of the distinguished men of the passing generation. Claparède was the descendant of a French Huguenot family, resident in Geneva since the Edict of Nantes. A paternal uncle, for whom Claparède was named, was a distinguished naturalist and professor at Geneva. Th. Fluornoy, the son of his father's sister and nineteen years Claparède's senior, always exerted a kindly interest in the education of the psychologist. Claparède's father was a pastor and writer, and active in the social and educational life of Geneva, as had been his father before him. Claparède was born March 4, 1873, in the house at Champel in Geneva where he lived most of his life and where he died, September 29, 1940.

As a psychologist Claparède contributed to many different fields, and was active in many administrative capacities. He states that he decided to become a psychologist when as a boy of eighteen he heard a lecture by his uncle Flournoy on the "The Souls and Modern Psychology." Flournoy suggested that a resident of Geneva should come forward and work in the field in which Charles Bonnet, also a Genevoise, had distinguished himself. Claparède decided that this should be his field. In his school years Claparède devoted himself to the natural sciences so far as they were presented in the University of Geneva. After completing the University, he spent a year in a special course in the natural sciences, including laboratory work and lectures in psychology with Flournoy. During this year he became aware that his synesthesia, which he had taken for granted as common to all,

was a relatively rare phenomenon and he therefore began to collect data. He distributed a questionnaire to his friends and went to Paris to consult Binet who was also working on the topic. Claparède's results were included by Flournoy in his *Audition colorée*, 1893. Claparède himself, in the *Archives of Psychology* for 1930, reported on the persistence of the colors.

After this year of general science, Claparède devoted himself to the study of medicine. He had one semester in Leipzig where he attempted to attend a *praktikum* in psychology under Külpe, but was barred by a limitation in the size of the class. He then returned to Geneva for the remainder of his course. His thesis was a study of the muscular sense in connection with a case of posthemiplegic hemiataxia. The medical degree was received by him in 1897. In 1896 he had married Hélène Spir, the daughter of the Russian philosopher, African Spir, whom he met in 1889, and to whom he had continued faithful in the intervening period. The year 1898 was spent at the Salpêtrière in a continuation of the study of ataxia. In Paris he renewed his acquaintance with Binet and began a study of the sensations of movement in collaboration with Henri which was never completed.

In 1899 Claparède returned to Geneva where he continued his studies under Flournoy. He was made docent and began to give courses in the psychology of sensation. In 1904, at the suggestion of James, Flournoy turned over the direction of the laboratory to Claparède. In 1908 he was advanced to Associate Professor and in 1915 he succeeded Flournoy who was transferred to a Professorship in the Philosophy of the Sciences. His teaching was inspiring, and he says it was directed to arousing in his students a desire to experiment for themselves. The work from his institute demonstrates his success.

Upon his return he developed a practice in neurology and psychiatry and also was attached to the clinics at the hospital. He continued a clinical session in psycho-therapy at the hospital dispensary until 1920. In the meantime, Claparède had become interested in the problems of animal psychology and published several theoretical articles on the possibilities of

scientific studies of animal behavior in various periodicals, including the *Revue Philosophique* and *International Quarterly*. He promised a volume in the Ebbinghaus series on the subject, but never completed it.

Throughout his life, Claparède devoted a great deal of his time and energy to the *Archives de psychologie*, which he founded with Flournoy, in 1901. A large part of the detailed work, especially with reviews and notes, fell upon him from the beginning. After Flournoy's death in 1920, he continued the work alone. Almost every volume has an article from his hands and every number has reviews, notes, and incidental contributions that add so much to the vitality of a periodical. Altogether, he had sixty-two major articles in the *Archives*. The various volumes constitute possibly his greatest contribution to psychology and give the best index to his many-sided mind.

Claparède apparently felt most interest in his theory of sleep as he returns to it in three separate articles. The statement of the theory was given in an address before the society of physics and natural sciences in Geneva and published in the Archives of the society in 1904. It was also reported at the Giessen meeting of the German psychologists and then was stated at length in the *Archives de psychologie* in 1905. The theory is familiar to most psychologists. It differs from the earlier ones in making sleep a positive rather than a negative reaction such as is implied in the toxic or other chemical theories. The occasion for the reaction is said to be disinterest in the surroundings. The reaction itself is referred to a positive inhibition of activity from some cerebral mechanism. The response is a protection against possible fatigue rather than the direct result of fatigue. The theory was designated as the biological theory of sleep. Claparède's interest in the theory is shown by the fact that in the *Archives* for 1929, twenty-five years after the first presentation, he published a summary of the criticisms and favorable reactions. He had previously reviewed the subject and presented his own view in *L'Année Psychologique* in 1912. Claparède also suggested as an appendix to his article on sleep in 1905 a theory

of hysteria that was developed on the same line. Hysteria arises from a disinterest in the environment that leads to a withdrawal from contact with it and so to a persistent inhibition. The theory has many points in common with later suggestions of Kretschmer and Max Meyer, and even of Kraepelin.

Claparède was instrumental in carrying out many investigations. One of the earliest was on the psychology of the witness published by Miss Borst in volume iii, 1904, in the *Archives de psychologie*. Claparède contributed much to the planning of the experiments and often referred to the results.

Of interest in demonstrating his predilection to the higher organization of consciousness was his contribution or interpretation of the work of Katzarof on recognition, published in the *Archives* in 1911. Katzarof affirmed that long experiments in recognition demonstrated that one appreciated an earlier seen object as familiar only when it was in some way connected with self. Claparède confirmed this theory by studies of a Korsakow's case of ten years standing. Many memories and responses could be shown to be retained but were not recognized. The patient would tell the interne she had never seen him before, and then would ask him when dinner would be ready. Claparède insisted that the associations were retained but that the capacity for higher organization was lacking. This he called connecting the event with the ego (*moiété*).

In his autobiography, Claparède says that he had started to write a theory of the will and that it was still lying in some drawer. This again shows an extension of the appreciation of the higher controlling activities that led him beyond the single connections in his discussion of associations. It also comes out in his implication of a real self in his autobiography.

Claparède's recent work of most general interest was his study of reasoning through the formation of hypotheses, and by the method of 'spoken reflection' or thinking aloud as an observer solved problems. The method was first used in 1917 and the fruits appeared in the article on the genesis of hypotheses in volume 24 of the *Archives*. Claparède's conclu-

sion is that the hypotheses are guided in their formation by the question and by a narrower or more extended constellation of ideas or intentions. The essence is to be found in a real anticipation scheme that guides the thought. The question grows from a need of the organism and thus brings thinking into the general biological system. There are always many different suggestions that present themselves before the true one appears. Most of these are within the range of the controlling groups of ideas, groups or complexes that may be narrower or wider. In addition there are occasional suggestions that may be irrelevant, even foolish. Still the guidance of the complex leads ultimately to the goal.

In this connection, although not so obviously in the article, Claparède emphasizes that there is a necessary connection between stimulus and response that is not dependent upon experience. This he called implication. In a letter to the writer, dated June 30, 1940, acknowledging a reprint in which his article on the hypothesis was discussed, he noted omission of mention of the notion. This statement of the principle may be quoted as it puts clearly one aspect of much of this thought.

> Assuredly, this notion (of implication) is still a little vague. But I think it a process of primordial importance in mental activity. Contrary to what has been admitted hitherto, I think, that the sentiment of *necessity*, of necessary connection, is not the result of acquisition by experience, but that it is primitive. We see that the infant acts as if each connection were necessary for him. If a single time the child is burned in touching the stove, he will act afterwards as if he thought the stove will necessarily burn him if he touches it again. In other words, it is not a product in his mind of a simple association between stove and burning, but an implication: stove implies burning.
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> This manner of thinking renders superfluous the attempts (otherwise sterile) to draw necessity from habit (as for example the inseparable associations of Stuart Mill). If one regards implication as a primordial and fundamental notion of our mental activity, phenomena such as induction, the primitive generalization of the child, the repetition of reactions which have once succeeded, are explained naturally. Without implication the individual will not know how to profit from experience.

Claparède's theory of thought in brief emphasizes the control of the suggestions by the preceding question and scheme. The steps in the solution of the problems as he gave them to his students are perception, reading, inference and then *tatonnement*, the striving for the hypothesis by successive trials. The actual attainment or discovery of the hypothesis escapes consciousness. The merit of the paper is a careful statement of the problem, an indication of the defects in the theories that have been offered, and a statement of the limits within which a solution must be sought.

On the theoretical side Claparède's system falls roughly in the functional field with a slightly different approach from that of the Chicago school, but essentially one with it in the insistence of the importance of control of adaptation by mind. Consciousness intervenes when instinctive or acquired automatisms do not suffice to solve problems. Intelligence is derived from the methods of trial and error of the lower animals. When developed the problems of readjustment that present themselves are solved by thought and not by chance movements. This utility of consciousness fits well into one of Angell's characteristics of functional thinking, although Claparède assigns it to Stern and does not mention the American school except incidentally.

Closely related to the problem of the development of consciousness is the 'law of becoming conscious' which runs: "The earlier and longer a relation has been in use, the later it is consciously perceived." The function of mentalization is to relate the processes which had been unconscious up to that moment to the representations due to acquired experience, and thus to permit the readjustment of action. This law was stated in a paper prepared for the Wittenberg conference and again emphasizes the functional trend in Claparède's thinking.

The educational and child psychology phase of Claparède's life was quite as important as his purely theoretical contributions. The beginning may be assigned to 1901 when he gave a lecture before the medical society on *L'école sur mesure* ("The school made to measure"). His interest waxed rather than

waned as time went on. He traces this interest to dissatisfaction with his own schooling that gave him little opportunity to learn the sciences. He attempted to apply the theories in a seminary of pedagogical psychology in 1906, but the professor of pedagogy protested that Claparède was trespassing in the field of education and the political head of the state department of education refused to send his future teachers to the university when it was subject to the new influence, so the seminary was abandoned.

After the success of his *Psychologie de l'enfant*, which was translated into ten languages, Claparède was encouraged in 1912 to found with Bovet a special school entirely independent of the university and of the state, the Institute J. J. Rousseau. The Institute was dedicated to experimental education and was at once a center for research, and a center for advanced teaching and for information and propaganda. It prospered from the beginning, attracting students from much of Europe and from America. While the war interfered with its growth, it held on through the period and grew rapidly after peace returned. After it proved itself, it was subsidized by the state, received an endowment from the Rockefeller Foundation, and was finally recognized by the University. Its success is now symbolized in stately buildings. There were centred the work of Ferrière, Walther, Mme. Antipoff, Piaget and many others. It has greatly extended the field of child study as presented in the *Psychology of Childhood* and in many related fields of education. Claparède's contributions to education as well as to psychology were outstanding.

Claparède was intensely loyal to his city and state. The name of the Rousseau Institute was an evidence of his appreciation of the historic place in thought of Geneva's great citizen. He also gives touching expression of the scientific prowess of the city in his monograph on Charles Bonnet, specifically written to remind the city of the early anticipation of modern work by that great naturalist and psychologist. Claparède was interested in the formation of a liberal party in Switzerland that should advocate a doctrine intermediate between left and right and was for a time a writer for its paper, the

Signal de Genève. He always took an active part in the civic affairs and contributed wherever it was possible to the development of an impartial rationalism in the affairs of city and state.

Psychologists knew Claparède best through the International Congress of Psychology. For a number of years he was permanent secretary and so had an important function in organizing the different meetings. His marked catholicity of interest and his freedom from bias towards any *ism* made him an ideal leader for the congresses. All of the many who met him at the Congress were impressed by his enthusiasm and friendliness and his efficiency. His ideal program was one with as much discussion of general principles as possible and a minimum of reports on special investigations. A meeting of men and of theories was his aim with as little as possible of formality.

An evaluation of Claparède as a psychologist must emphasize the breadth of his learning and the many fields to which he made contributions. He speaks of himself as likely to be inhibited by conflicting tendencies between the romantic and the classical tendencies of Ostwald, the desire to be creative and to explore ever new fields, and the desire to be logical and orderly in thought. He asserts that a work on intelligence to complete the *Psychology of the Child* was not written because he could not satisfy himself with the arrangement of the material. He thought of his own work as consisting mostly of preliminary exploration, on the one hand, and of organization of the material in different fields, on the other. Both of these functions he performed admirably, and they are valuable for any science. In addition he did make a number of discoveries and theoretical contributions that will undoubtedly constitute permanent additions to the basic theories and facts of psychology. Psychology is the richer for his life.

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STATISTICAL FREQUENCIES AS DYNAMIC EQUILIBRIA

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I. Introduction

The growing importance of statistical methods in both physics and the social sciences makes a complete understanding of their implications and use imperative, and the large controversial literature on the meaning and use of the theory of probabilities in science shows that this need for clarification is not being overlooked. The social sciences use statistics when they are—perhaps only temporarily—ignorant of the nature of determinants which would, if known, adequately account for conduct; but it is the principle of indeterminacy, of *necessary* ignorance, that has forced statistics into physics. The present paper is a plea for more determinism in statistics, for a causal interpretation of probabilities in science and a resort to indeterminacy only when determinacy is known, at least for the time being, to be impossible.

The paper is concerned with the saying of three things. (1) First, it reminds the reader that the Principle of Indifference is not a pseudo-empirical means for getting knowledge out of ignorance, but at best a property of a particular form of model which may or may not be applicable to specific data. (2) Secondly, it notes that the Principle of Independence, the supposed mutual independence of the events in a statistical series or collective, can exist, if the series converges upon a given frequency, only if the number of cases is indeterminate or infinite, and it argues that the number of cases in the law of large numbers must be finite and determinate if the statistical model is to be of scientific use, since a model must have definite dimensions. It follows that, when models, representing convergence within a finite, specific number of cases, are known to apply to a given statistical series, then the in-

vestigator is faced with the problem of discovering the nature of the mutual interaction among the events which causes the series to converge. (3) Finally, the paper selects seven classical instances of actual supposed convergence, and points out how they imply the existence of interaction among the events and, in as far as possible, it indicates the general nature of the equilibrating forces which would make the events tend toward a limiting frequency. In this manner the paper deplores a complacency with statistical indeterminacy and suggests that the unknown determinants would often be discerned if the investigator would realize that their existence is implied by the convergence of a collective.

II. The Principle of Indifference

The Principle of Indifference, as Keynes formulated it,[1] requires that the probabilities of different events should be equal when the evidences for the occurrence of each are equal. The use of the Principle depends, however, upon the definition that is accepted for probability, upon whether probability is regarded as axiomatic, or subjective, or a frequency.[2] The Principle runs, moreover, into difficulty when the evidences for events are equal to one another and also to zero, for then equal probabilities appear to be derived from equal ignorances.[3]

(1) The *aprioristic* view works at the formal level of the model. The mutually exclusive possibilities are laid down, and, since there is no reason for weighting them differently, they are assumed to be equally likely, so that their consequences can be worked out by theory of probabilities. The discussion of the probabilities in games of chance is generally on this formal *if-then* level: *If* the elementary events are equally likely, *then* the probability of specified compound events would be so and so.

[1] J. M. Keynes, *A treatise on probability*, 1921, 41–64.

[2] *Cf.* the classification of concepts of probability by M. Fréchet, The diverse definitions of probability, *J. Unified Science* (*Erkenntnis*), **8**, 1939, 7–23.

[3] Thus G. Boole, *An investigation into the laws of thought*, 1854, 368–375, esp. 370, called the principle 'the equal distribution of ignorance,' and J. von Kries, *Die Principien der Wahrscheinlichkeitsrechnung*, 1886, 51–15, esp. 6, called it 'the principle of insufficient reason.'

Nevertheless, even at the formal level, difficulty with the Principle of Indifference sometimes develops because some other formal rule relates two terms in such a way that they cannot both be distributed in accordance with the Principle. For instance, if x is normally distributed, then x^3 and $1/x$ cannot be, although the application of the Principle of Indifference to any one of the three variables alone might give a normal distribution. The linear heights and the weights of symmetrical homogeneous objects cannot both be normally distributed, since the weight would vary with the cube of the height. Bertrand's paradox arises because the Principle of Indifference gives different results for the ratio of water to wine in a mixture, and to its reciprocal, the ratio of wine to water in the same mixture.[4] In short, the principle of indifference cannot be applied simultaneously to quantities that are non-linearly related.

It is quite usual for empiricists, defining probability as frequency, to take over the Principle of Indifference and its consequences as a formal model, and to determine the goodness of the model's fit in a particular case or even sometimes to assume that it fits without bothering to test it. That is, after all, the history of the theory of probabilities, which was in part developed by savants in the interests of princes' games of chance. In such games experience with cards and dice and coins indicates that the conditions under which the model will fit 'well enough' can be roughly intuited, although this intuition of necessary conditions, as we shall see later, is rarely explicated. It is very important for us, however, to keep the rules of the formal model distinct from its empirical relations. At the formal aprioristic level the Principle of Indifference is only a property of the model and means nothing whatsoever about empirical facts.

[4] Bertrand's paradox is this. If the ratio of water to wine lies between 1 and 2, then there must be equal likelihoods of its lying between 1 and 3/2 and between 3/2 and 2. But, if the ratio of water to wine lies between 1 and 2, the ratio of wine to water must lie between 1 and 1/2, and according to the Principle of Indifference there would be equal likelihoods of the ratio lying between 1 and 3/4 and between 3/4 and 1/2. Yet these two results are contradictory, since $3/4 \neq 2/3$ (the reciprocal of 3/2). *Cf.* R. von Mises, *Probability, statistics and truth*, 1939, 114–117; and the similar example in Keynes, *op. cit.*, 45.

Common sense habitually errs by confusing the model with the data. Both the casual scientist and the man of the street will say that, if there are two possibilities, *A* and not-*A*, and that is all one knows, then *A* and not-*A* are equally likely (which is correct on the aprioristic view of the model) and will happen equally often (which is quite wrong when there is no empirical ground for establishing the fit of the model). Suppose *A* is *Six* on an ordinary die thrown in the 'ordinary' manner (whatever that is!). *Six* and not-*Six* may be equally likely on aprioristic grounds, yet they do not turn up equally often.

(2) The *subjective* view is that probability varies with the weight of the evidence, so that the same events may be differently probable to different persons for whom the evidence has different weights. Although there has been much discussion with the contrary intent,[5] it seems to me that at this simple level there need here be no appeal to Indifference. The relative weights are whatever they are. Perhaps they are determined by the betting odds which the subject will accept or perhaps by some other subjective method. Equal likelihood becomes then just a special case of all other relative likelihoods.

A difficulty arising here occurs because such probabilities have in turn their own probabilities. Evidence may be much or little as well as equal or unequal. A slight preponderance of slight evidence is not the same as a slight preponderance within a great mass of evidence. In the limiting case, where the evidences are equal because all the evidence is zero, one has minimal assurance for consequent action, even though action may be required. My point here, however, is that one does not return to a Principle of Indifference merely because evidence is insufficient. Nor does one get assurance out of ignorance, any more than he gets knowledge out of ignorance. The problem of assurance is subjective, and one 'gets' only whatever is there to get.

(3) The *frequency* theory of probability simply identifies probability with relative frequency. This is the view that

[5] *Cf.* Keynes, *op. cit.*, 41–64.

appeals to the scientist and to the positivist, the view that has been so adequately expounded by von Mises.[6] Scientifically it provides the 'best' operational definition of probability because it is the most precise. Always it can be given numerical meaning. It is true that subjective assurance can also be quantified, as by betting odds, but never so precisely. Frequency is, therefore, the only empirical definition to make full use of the precision of the mathematics of probabilities employed in the construction of models. At any rate this definition of probability is the one that is most germane to the argument of the present paper about convergent statistical frequencies.

The Principle of Indifference belongs no more to the frequency theory of probability than to the subjective theory. One simply does not get the knowledge of a frequency out of ignorance about a frequency. What one does is to conclude from known conditions to observed frequencies by the process of induction, and then, according to an appropriate model furnished by theory of probabilities, to conclude further to other frequencies, perhaps testing the fit of the model by subsequent observation. This view is essentially the one so clearly presented by von Mises.

In summary, we can say that the Principle of Indifference is a postulate that is applied axiomatically in the mathematics of probability and thus belongs in the mathematical models available for use in connection with the other two theories. Only in this manner is it validly employed in connection with empirical data. Of course, the fit of these models to observed subjective probabilities or to observed frequencies can be tested, but there is no other way in which the 'correctness' of the Principle can become subject to validation. In other words, there is no way in which equal likelihoods can be got into the universe of discourse except by (*a*) postulating them

[6] von Mises, *op. cit., passim.* In supporting the frequency theory von Mises rightly ignores the Principle of Indifference, yet the Principle is insidious and I cannot feel that von Mises has got completely free of it. He is too pleased when he finds a model, based upon it, fitting an empirical case, as in his verdict on Marbe's problem, 198–201. *Cf.* also 237–240, 248–250, 280, 283. I am not, however, writing this paper to quarrel with von Mises who is, at this point in the argument, on my side. *Cf.* H. Reichenbach, *Experience and prediction*, 1938, 297–357.

or (*b*) observing them. If the equal likelihoods are observed, then they must be (b_1) subjective assurances or (b_2) relative frequencies. To determine empirically whether postulated likelihoods lead to certain predicted assurances or frequencies is simply to determine the fit of a model in a particular case: Thus the Principle of Indifference gains no special interest among all the other relative likelihoods. There is, it is true, the special empirical problem as to how subjective assurance is correlated with consequent frequency, but that is an empirical problem and not a case of testing the Principle of Indifference. Hence it appears that empirical science can get along very well without special attention to the Principle, except as it is postulated in the mathematical logic of the theory of probabilities which furnishes formal models for observed correlations.

Since this is no new doctrine, it seems fatuous to spend so many words in dismissing the unimportant, but the insidiousness of the Principle of Indifference justifies insistence. Let us get outselves quite clear, therefore, before we proceed, about that major instrument of the probability theory, the tossed coin. It has *Heads* and *Tails* only, and *Heads* = *not-Tails* and conversely, the two being the mutually exclusive terms of a disjunction. Are *Heads* and *Tails* equally likely? No! Not on the basis of any information already printed in this paper. If the coin is homogeneous as to specific gravity and symmetrical as to weight, are they equally likely? No! These are negative conditions, statements that weight and shape will not effect the separate throws of which the series are composed. What then can we do about such a series? (*a*) We can postulate equal likelihood for *Heads* and T*ails*, and work out, for instance, the probability of seventeen iterations of *Heads* on the basis of the postulation. (*b*) We can assume that this model applies to subjective assurances and see whether it fits, whether a man would give odds of 131,072 to 1 against a run of exactly seventeen *Heads*, no more, no less? Probably he will not, unless he is a confirmed mathematician who settles his feelings of assurance by mathematics. If he will not, we can find out what odds he will give, and how they

differ from the odds another man will give. (*c*) We can assume that the model applies to objective frequencies, and see whether it fits there. It may or it may not. It is easy to fix the conditions of tossing so that *Heads* will never turn up, or will always turn up (*vide infra*). And that is a fair proposal in this argument, because the exact mechanics of tossing is so rarely expressed in these statements of conditions, that I am free to introduce any system of tossing that I want. It is not fair to specify that the tossing shall be 'without bias' because *without bias* is either another negative condition that determines nothing, or else it prejudges the case, assuming the rejection of all cases that do not fit the model, when the fit of the model is the point at issue.

If all the statements of the preceding paragraph were at the start self-evident to the reader, then indeed, from his point of view at least, the preceding discussion has been fatuous. I think, however, that most persons who work with statistical methods would not have found them self-evident, although I hope they may by now have accepted my logic.

III. The Law of Large Numbers

It is a property of a statistical series, a collective, that, as the number of cases is increased, the relative frequency of a particular event approaches a specific value as a limit. If this condition is not satisfied, then the event cannot be treated under the theory of probabilities, and it has not, to use von Mises' terms, the properties of a collective. Now it seems a valid scientific generalization that such collectives do exist, that relative frequencies are predictable when the individual occurrence remains unpredictable. We know—or we think we know—from experience (and not from the Principle of Indifference) that the coin comes *Heads* about half the time if it is thrown often enough, and we know that more than one throw is required, since it cannot come half *Heads* in a single throw. Experience teaches us, moreover, that under many conditions which can be set up, a run of two or three *Heads* is not unusual, whereas a run of twenty or even ten *Heads* is exceptional. If one points out that coin tossing can be so

controlled as to defeat this expectation, then common sense appeals to the records of roulette, to the mortality tables, or to some similar situation where commercial solvency attests the validity of the law of large numbers.

Now I have no quarrel at all with the thesis that frequencies in a statistical series are convergent and that they come under the rule of induction, being predictable without undue error on the basis of past experience. It is, indeed, the purpose of this paper to ask what is implied by the fact that such predictions can be made. The law of large numbers does not, however, express any such simple principle. It states only that the probability, that the relative frequency will not deviate more than a specified amount from some specified frequency, increases as the number of cases increases. Hence under this law the ultimate frequency is determinate only for an infinite number of cases. Every exception to it fits in under the infinitely inclusive rule. If I were to throw 36 *Heads* in succession with an 'unbiased' penny and do it on the 50-billionth attempt, then everyone who knew the mathematics of the situation would say, "Well, it's about time!" But, if I were to do it on the first attempt, then everyone would look for bias, and, if bias could not be positively established, only then would they admit, "But it ought to happen once in 68,719,476,736 times." If in the second 36 throws I again got 36 *Heads*, there would still be a statable frequency with which I 'ought' to get two such events in succession. It does not seem to me to be a good model which fits every conceivable set of occurrences.

The law of large numbers is especially important because it has a close relation to the Principle of Independence. Statisticians usually suppose that the events of a collective occur in complete independence of one another. Thus the expectation of any event is not affected, so they say, by the outcome of preceding events. A run of *Heads* does not increase the probability of *Tails*. At first thought it seems quite impossible to reconcile this Principle of Independence with the notion that relative frequency approaches a limit, for, if there have been 'too' many *Heads*, then there must be

a subsequent increase in *Tails* if the limit is to be approached. The Law of Large Numbers, however, takes care of that difficulty. No finite iteration of any event is large when compared with a 'sufficiently' large number. The word *sufficient* turns the trick. So long as it remains a word of indefinite specification, however, just so long will the concept of a probability series, a collective, remain vague and half-way meaningless, simply because there is no conclusive test of the existence of such a series.

Von Mises, whose strict operationism led him to champion the frequency conception of probability, is here not so rigorous. He makes his conviction plain that the properties of a collective are realized only for a 'sufficient' number of cases, and *sufficient* does not mean a definite finite quantity. While he has not directly espoused the Principle of Independence, nevertheless he has specified it indirectly in his requirement that there be no change in the probabilities of a collective when there has been a selection of its elements in respect of their places within the probability series. In consequence of his holding to both of these principles, he is forced also to espouse the principle of indeterminacy, and then ultimately to deplore the inertia which leads scientists to hold so tenaciously to causal determinism. To those who cannot take time to read von Mises in order to see why the properties of a collective and the law of large numbers lead to the principle of indeterminacy, I can recommend only that they read the rest of this paper where the converse relationship is exhibited: that determinism is gained by an abandonment of the law.[7]

Certainly there is a deterministic statistics! It surrenders the Principle of Independence along with the Principle of Indifference. It goes back to observation and builds upon it inductively. Thus it accepts the frequency theory of probabilities in the sense that the theory of probabilities provides the models which the observed frequencies are to fit. For a law of 'sufficiently' large numbers, this conception substitutes a law of relative frequencies converging upon a limit within a determinate number of occasions.

[7] von Mises, *op. cit.*, esp. 243–307.

In other words, we set up statistical models in which the relative frequency of specific events varies between 1.0 and 0, converging in a specific number of cases upon a specific relative frequency. Such a model is subject to test as to its goodness of fit to the observed frequencies. Both the limiting frequency and the number of cases are thus observed data. That a coin when tossed by a given machine should yield from 48 to 52 *Heads* in 100 throws is an example of specifications which constitute a model; and, if the model does not fit well enough, we can change it, either as to the limiting frequency or as to the number of cases. In many instances we shall find that increasing the number of cases will make the model adequate where it was inadequate before, but that will not always be true. Observation is the arbiter of the fit of a model, and repeated observation under the principles of induction is the only way to determine the reliability of the fit.

IV. The Frequency Matrix

Figure 1 is a frequency matrix for the first 36 occasions of the occurrence of the simple disjunction *A* or not-*A*. It

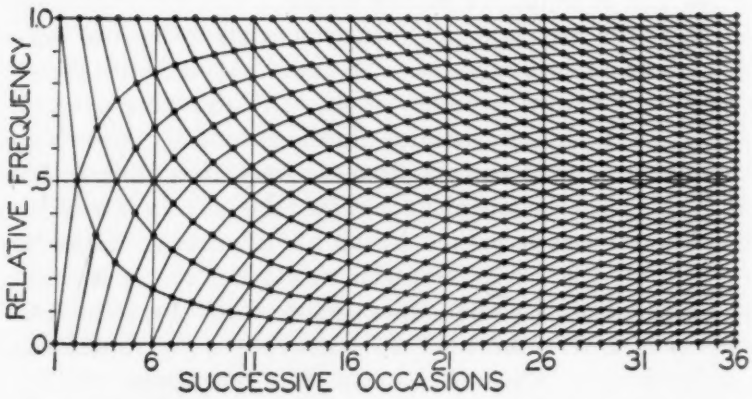

Fig. 1. Frequency matrix for a simple disjunction when successive events are independent. There are in this matrix 68,719,476,736 sequences of relative frequencies (paths through the diagram) from the 1st to the 36th occasion.

gives, as dots on the diagram, all possible relative frequencies, from 0 to 1.0, of the occurrences of *A*. On the first occasion *A* can occur only 'never' (0) or 'always' (1). On the second

occasion the frequencies can be 0, 1/2 or 1; on the third occasion, 0, 1/3, 2/3, or 1; on the fourth, 0, 1/4, 1/2, 3/4, or 1; on the 36th, 0, 1/36, 2/36, 3/36, . . . 35/36, 1. Thus one can trace the sequence of relative frequencies of *A* through the matrix from left to right for the first 36 occasions. For any frequency at any occasion, there are only two paths leading to the next occasion: one leads up and applies when *A* occurs on the next occasion; the other leads down and applies when *A* does not occur. Every possible sequence lies in the matrix.

There are 68,719,476,736 sequences through this matrix. No one, in the present state of our ignorance about *A*, can be known to be more frequent than another, but that is not at all to say that they are equally frequent. Lacking cogent reasons we just do not know about the relative frequencies. Of these many paths 9,075,135,300 end at .5 at the right of the diagram, and only one path ends at 1.0 and another one at 0. The number of paths that lead to the 37 frequencies represented on the ordinate for occasion 36 are the numbers of the coefficients of the binomial expansion for the 36th power, a 'normal' distribution of frequencies. Because there is only one sequence leading to 1.0 at the right and nine billion ways of getting to .5, most persons would say that we shall more often end up at .5 than at 1.0. That, however, is the sheer nonsense that comes from applying the Principle of Indifference aprioristically to an empirical case. If we do not know about actual occurrences, we simply do not know about them. Perhaps *A* is that the President of the United States will not die tomorrow. At once 1.0 at occasion 36 becomes more frequent than .5. Perhaps *A* is that a *Six* on a die will turn up. Then .17 is under certain conditions more frequent than .5. The number of sequences leading to a final relative frequency has no relevance at all to the problem in hand.

The frequency matrix of Fig. 1 must be clearly understood as representing the occurrence of an attribute whose relative frequencies are unknown in advance and are quite free of the Principle of Independence. In interpreting the diagram we must remember that there is no presumption which favors one sequence more than another or as much as another or as little

as another. We must have cogent reasons (usually in the form of past experience with frequencies) before we can prefer one path to another or not prefer it. It may be remarked, moreover, that the matrix, being finite, represents the beginning of a collective, or it includes all collectives in which 36 is a 'sufficiently large number.'

It should be observed that the free situation represented in Fig. 1 gives a delusory convergence. Frequency changes less and less rapidly as the number of occasions increases. The most that the frequency can change on the fourth occasion is .25; on the tenth occasion, .10; on the 100th occasion, .01. A great deal of the common belief in the law of large numbers must be founded on nothing more than this simple fact that relative frequencies have to change more and more slowly as the number of cases increases, whether a limit is being approached or not.

Finally we may note that Fig. 1 represents the case of drawing balls from an urn and counting the white balls drawn, when there are both white and black balls in the urn and the ball drawn is put back after being recorded. Such a system, so far as my description of it goes, is completely indeterminate as to whether it converges on a frequency or not and as to what, if it does converge, that frequency would be—and this statement holds without regard to the relative numbers of white and black balls in the urn. If the drawings approach any relative frequency as a limit, then that approach is due to those conditions which determine whether a white or black ball is drawn, to the differential determiners which are so often unknown. For instance, in an urn containing 8 black balls and 2 white balls, one could get 100 per cent of white balls by the simple expedient of looking at the balls before they were drawn and choosing always a white. If, without looking, one were to get only 20 per cent of white balls, why then that would be a mystery until one learned why.

The confusion that arises at this point is due to an asymmetry of scientific curiosity. If the distribution of a variable turns out to be 'normal,' we say that it is 'homogeneous' with the variability due to 'chance,' and relax at once into

complacency. If the distribution turns out to be, let us say, bimodal, then we say it must be 'heterogeneous,' requiring an 'explanation.' Why do we hunt for causes in the one case and not in the other? (And did anyone ever discover a bimodal distribution of a homogeneous variable and rest content with the finding?) The bridge hand of 13 spades seems to call for explanation as if it were 'improbable' and must have been caused by special conditions, whereas any bridge hand at all that ever was dealt is just as 'improbable,' just as reasonable a ground for wonder, reminiscence and anecdote.[8] Science could get along very well, I think, without the use of the words *chance* and *homogeneous*, and especially without that phrase, that potent anesthetic for curiosity, *due to chance*.[9]

V. Equilibrated Frequencies

If a frequency series approaches a limit in a specific number of cases, then the events cannot be independent, but are interrelated, so that the occurrence of one makes 'more probable' the occurrence of another. Fig. 2 is a matrix for the convergence of the frequency of one alternative A upon a relative frequency of 1/2 in 36 occasions. If initially A turns up 18 times in succession, then it must be inhibited from turning up again if the frequency is to be 1/2 at the 36th trial. This is the case of the urn with 18 white balls and 18 black balls, where the drawing of white balls is recorded and the balls are *not* put back after being drawn. The diagram might be taken tentatively as a set of models for the frequency of *Heads* in coin tossing, a set to be revised if the method of tossing were not such as to give this variation within these limits. The conditions of tossing like other scientific conditions ought to be kept constant. If they are kept quite con-

[8] The theme that no one sequence of events is any more improbable than any other, even though it happens to make some special sense, like a chimpanzee's typing out *Oliver Twist* by accident, has been put forth properly, if humorously, by R. Maloney, Inflexible logic, *The New Yorker*, Feb. 3, 1940.

[9] An example of an uncritical and, I think, undefinable use of the terms *random* and *homogeneous* occurs recently in Q. McNemar, Sampling in psychological research, *Psychol. Bull.*, 37, 1940, 331–365, esp. 335–339, 342, in spite of the fact that he is writing didactically in an effort to increase the sophistication of the users of statistical methods. See also the reference to Rhine, note 11, *infra*.

stant, as they easily could be, the model could be made to fit perfectly. We should have only to toss the coin with a machine and to decide whether to turn the coin over or not when returning it to the machine for another throw. Let us suppose that the machine is accurate and that it always turns up the side of the coin that was down in the machine before the toss, and that the coin is not turned over when replaced in the machine. Then the model for the frequencies of *Heads*, if the coin be first put in the machine as *Heads*, is the sequence 0, 1/2, 1/3, 1/2, 2/5, 1/2, 3/7, 1/2, 4/9, 1/2, . . . converging in this fashion upon 1/2.

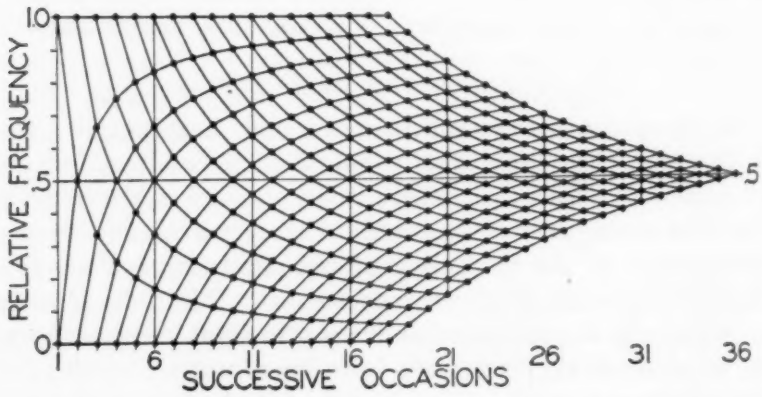

FIG. 2. Frequency matrix converging upon one-half. The diagram is for interdependent events with the limiting frequency of one-half reached on the 36th occasion. There are 9,075,135,300 sequences in this matrix.

If we know nothing about the differential determinants between the alternative events, then all we can do is to experiment until we know whether the series converges and what model fits it within the allowable limits of tolerance.

Figure 3 is the matrix for the sequences which end in 1/6 after 36 occasions, as, for example, the frequency of *Sixes* in the throws of a die, when the die is thrown so that the series does thus converge upon 1/6. Of course, a die can be thrown so that the frequency of *Sixes* will approach 1/2, and then the model for it may lie in the matrix of Fig. 2. It all depends upon the differential determiners of its faces.

It should now be plain that, if frequencies approach a limit, they are striking some sort of dynamic equilibrium as the result of the forces which operate between the successive events. In the following paragraphs we get some idea of the kinds of equilibrating systems that operate or might operate to fix statistical frequencies at definite amounts.

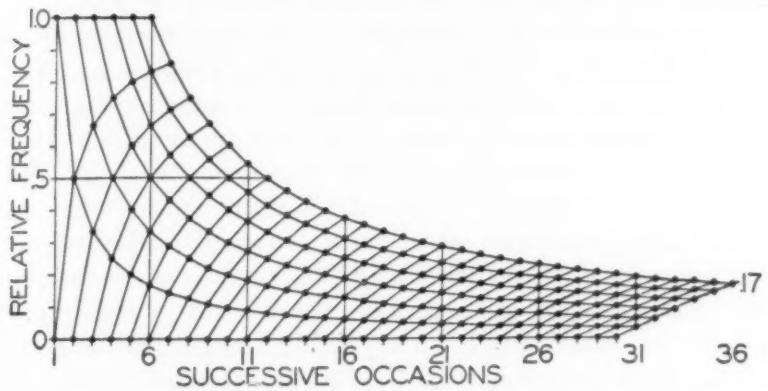

FIG. 3. Frequency matrix converging upon one-sixth. The diagram is for interdependent events with a limiting frequency of one-sixth. There are only 1,947,792 sequences in this matrix.

Coins and dice.—There are three possibilities: (1) we can be wholly ignorant of the determiners of the particular events; (2) we can know from past experience that the relative frequencies converge upon a constant relative frequency in a specific total of occasions, although we do not know 'why' they converge; or (3) we can know wholly or in part about the determiners of the particular events which cause the convergence.

(1) The case of complete ignorance about the properties of the series can have no scientific interest. We can appeal only to the Principle of Indifference, and knowledge conditioned upon ignorance is a delusion. We have already seen that coins can be thrown so that the relative frequencies will converge upon any desired frequency, depending on the dynamics and rules of tossing.

(2) It is very generally believed that there is good empirical evidence that a coin will in the long run come up equally often

Heads and *Tails*. If this law of the coin is true—in spite of the fact that we can specify conditions of throwing under which it would not be true—then it follows that persons can by intuition recognize the conditions under which equal frequencies of *Heads* and *Tails* would be approached. We must exclude the completely circular case where 'right' results are accepted and 'wrong' results are scrutinized with the hope of finding a reason for their exclusion. The *aprioristic* intuition means, then, that we can choose certain situations in which our prediction of fifty per cent *Heads* in a certain number of trials is verified approximately. We can have such a situation, but, when we have it, what can we do with it? We cannot test the 'laws of chance,' for there are no empirical laws of chance except correlations between conditions and consequent frequencies, and here the conditions are intuited and not specifically generalized. We can, of course, try to analyze the situation in hope of gaining a more precise specification of the determiners of the limiting frequency, but this attempt is seldom made because of the general belief in the indeterminacy of the events in a statistical series. If we make the attempt and succeed, we pass almost certainly into the next case.

(3) When the determiners of the specific events are known, as they would be with a carefully constructed tossing machine, then we know everything and have nothing to learn.

The general problem becomes clear if we consider the case of a biologist who wishes to examine the laws of Mendelian inheritance as to whether the genes combine 'by chance,' and to parallel these matings of cells by the throwing of coins or dice. The simplest case would be the relation of a Dominant to a Recessive character, with Dominance equated to *Heads* and Recessiveness equated to *Tails*. A homozygous cell would, therefore, be represented by a coin with two *Heads* or two *Tails* and a heterozygous cell by an ordinary coin with one *Head* and one *Tail*. There is no need to enter into details, but, supposing that the results of mating experiments match the results of double throws of these coins, what has been gained? We have fitted a coin model to the biological situa-

tion, but we know nothing much about the coin model. We have to observe the frequencies of both the matings and the throwings, and the mathematical model of frequencies was all that we ever needed. Tossing coins has done us no good at all.

It is the same with randomness. No dice box ever gave randomness with certainty. You have to know first what randomness is in order to be sure that the dice give it. If you know what it is, you do not need to throw the dice. The safe way to get randomness with a die is to make out a table (model) for random throws, repeatedly pick up the die, and every time put it down in accordance with the table!

Cards.—The same logic applies to the use of cards. Shuffling is certainly not necessarily 'random' merely because it keeps the shuffler ignorant of what he is doing. If a shuffler aims at dividing the pack in two and at alternating the cards from the two halves, then he aims at a method which would bring a pack of 52 cards back into its original order every eighth shuffle! This is to me an astonishing discovery. A pack of 64 cards, divided in half, with the cards alternated and with the same side always providing the first card, will return to its original order in six shuffles; 16 cards return in four shuffles, 12 cards in ten shuffles.[10] In other words, 'good' shuffling depends upon the failure of the shuffler to realize his intention, since it seems certain that most shufflers try to divide the pack evenly and to alternate the cards. Thus it comes about that, to understand any scientific situation involving shuffled cards, we need to understand the nature of those determiners that make the shuffler deviate from his intention. It is meaningless to say that shuffling—when the system determining the order of the cards is left indeterminate—gives a 'chance' distribution of the cards. Unless we know the rules that determine the order of the shuffling, we remain ignorant of essential conditions.

[10] The rule is this. The deck is evenly divided and the cards are alternated, with the card previously on the bottom put down first. Let n = number of shuffles to bring the deck back into the original order; d = number of cards in the deck and an odd integer; c = another odd integer, a constant for a given d. Then $cd = 2^n - 1$. Packs with an even number of cards are taken care of by the fact that n is the same for $d+1$ (even) as for d (odd). I am indebted to my colleague, Dr. S. S. Stevens, for this general formula.

The barrenness of recent research on extra-sensory perception has thus been due, I think, to the failure of Rhine and his associates to become interested in the nature of shuffling.[11] These investigators have tended to apply the Principle of Indifference to frequencies, to assert, when a pack consists of five kinds of cards in equal numbers, that guesses about particular cards must be right one-fifth of the time. Thus, when the guesses are right two-fifths of the time, they are amazed and feel constrained to call for explanation upon an hypostatized principle named *extra-sensory perception* or *ESP*. If ESP makes the cards come right two-fifths of the time, what made them come right one-fifth of the time? By what right are these men complacent about this mysterious entity which they call *chance*, while they demand our concern about the other mysterious entity which they have christened *ESP*? ESP is a mystery because chance is a mystery, and, if we are going to boast about our ignorance of the one, we had better begin boasting about our ignorance of the other. What expectation is there that they will really discover the conditions which make a frequency 1/5 change to 2/5, when they know nothing (and care nothing) about what makes the frequency 1/5? Here is indeed a situation where the use of statistical method leads to an assured complacency and blindness to the basic problem.

Galton board.—Von Mises has noted that the Galton board shows the properties of a collective, and he describes a Galton board in which the successive events would seem to be entirely independent.[12] Pins are arranged in rows in an inclined plane.

[11] J. G. Pratt, J. B. Rhine, B. M. Smith, C. E. Stuart, and J. A. Greenwood, *Extra-sensory perception after sixty years*, 1940. The distinction between the use of the Principle of Indifference in the mathematics of a model whose fit is subject to test and its aprioristic application to frequencies is subtle, but it is my conviction that an improper use of the Principle occurs repeatedly throughout the book. *Cf.* esp. Chap. 2 on mathematical methods and Appendix 1. For instance: "The method commonly used in evaluating a particular set of data is to calculate the probability that a score as high or higher than that obtained would occur in that number of calls. This probability is based on the number of times such a score would occur, if chance were the only factor operating, in a large number of experiments, each consisting of the same number of calls as the series to be evaluated" (p. 29); or "The composition of the deck is the same for all calls, therefore the probability of success is constantly 1/5" (p. 363), a sentence which occurs in a context that equates probability to frequency.

[12] von Mises, *op. cit.*, 327f.

The distance between adjacent pins equals the distance between adjacent rows, and the pins are staggered so that the pins of odd rows come half way between the pins of even rows, and conversely. A steel ball of diameter equal to the horizontal distance between pins is started at the top in the middle and allowed to roll down to the bottom. At each pin it makes a choice between left and right. The process is repeated many times. If the Principle of Indifference governed, then the frequencies of the places where the ball arrives at the bottom of the plane in a large number of trials should show a normal distribution.

Some Galton boards are arranged so that a charge of shot, falling through a hopper, strike against each other and produce a symmetrical unimodal distribution because of equilibrating interaction among the shot. Of von Mises' case it can only be said that the conditions which he specifies are insufficient conditions for any kind of a distribution. The simplest Galton board would have a single pin, with the shot, dropped one at a time, turning up in equal numbers to the right and the left of the pin. Here the sufficient condition lies, not at all in the construction of the board, but in those factors which determine whether the shot shall strike to the right or left of the center of the pin (or remain balanced upon the pin!). The case of the shot is like the case of the coin. It is the operation of this approximate ignorance that Galton called "the wonderful form of cosmic order expressed by the 'Law of Frequency of Error.'" [13] The board proves that a shot's falling to the left increases the tendency of subsequent shot to fall to the right, and conversely. Just what is the nature of this equilibration?

Gas.—The kinetic theory of gases is often said to be founded upon statistical theory. For the most part, however, the development of the theory is indubitable mathematics and does not involve the theory of probabilities. On the other hand, there is a hint of the Principle of Indifference when it is said that, since the direction of motion of the molecules is unknown, we must regard them as moving equally in all direc-

[13] F. Galton, *Natural inheritance*, 1889, 63–66.

tions, or, for the purposes of mathematical treatment, one-third of them moving in each of the three dimensions.

It would seem, however, that we have to do here with a case of dynamic equilibrium and not of the Principle of Indifference. Suppose, for example, that all the molecules were at some instant moving in only one direction. We should not expect them to continue without the collisions which would tend to alter their directions. Any alteration would diminish the uniformity of direction. Let us imagine the molecules free to move in two dimensions only, NS and EW, and let us imagine that they are all moving NS. Any deviation must be in the direction of increasing the total EW components, since the NS components, being already one hundred per cent, cannot be increased. As long as the NS components are greater than the EW components, we should expect the latter to increase, with equilibrium established at equality. Gas pressure is equal in all directions because only thus is the gas in dynamic equilibrium.

Radioactivity.—Lindsay and Margenau discuss the common view that the autocatalytic law of atomic disintegration is a statistical law with the particular events independent and thus indeterminate. "The number of atoms disintegrating is proportional to the number of atoms present," so that the process slows down as it proceeds. Yet they say: "On the *average* each atom is transformed after it has existed for a perfectly definite length of time, *e.g.*, many millions of years in the case of uranium; . . . the explosion of one atom, which occurs with a violence having no analogue on a macroscopic scale, leaves its neighbor undisturbed until perhaps after millions of years the neighbor succumbs to the same fate. The life of an atom appears to be regulated only by chance." It has "an isolated fate." [14]

It is hard to see why an atom, dying because of atomic over-population, as it were, suffers 'an isolated fate,' even though its neighbors, making up in the formula the N which required that this atom and so many others had to die, may live on undisturbed for a few million years. I should

[14] R. B. Lindsay and H. Margenau, *Foundations of physics*, 1936, 188–201, esp. 190.

suppose we had evidence here, either for some kind of dynamic interdependence among the atoms, or else for such an interdependence in formation of the radioactive substance as would be necessary to preestablish the atomic life-spans in accordance with the autocatalytic law. Lindsay and Margenau seem to appeal here to 'chance' because this atomic action is 'isolated' in the sense that it is at a distance. On that principle the operation of gravitation would be statistical.

Mortality.—While it is not possible to suggest what dynamic principle controls the deaths of atoms, it is quite clear that there are many such principles determining the deaths of human beings. The mortality tables—the argument of the man of the street that the commercial solvency of the insurance companies proves the lawfulness of chance—are certainly the result of equilibration. Everyone who dies makes life a little safer for those who live on. Everyone who lives jeopardizes his neighbors an infinitesimal amount. If there is an epidemic, social forces are brought into play to diminish deaths and to keep the mortality tables right. If there are 'too' many deaths on the highways, the speed limit is reduced—for the sake of the mortality tables. Medical science strives to prolong life and probably ends by increasing the maximal speed allowed on the highways. These are, however, only the lesser determinants of variations on the surface. Underlying them are the basic laws of population, economic and biological, which control the size of a population under certain conditions. Conditions can change, it is true, but so can the mortality tables. There are many equilibrating forces that tend to stabilize death rates and they must be interdependent. The superstition that the mortality tables validate some law of chance is hardly more than the superstition that my death has nothing to do with yours, an absurd idea.

Sex.—The approximate constancy of the sex ratio is so far from having its determinants known that the Principle of Indifference is easily accepted in respect of it. Boy babies and girl babies are supposed to come equally often simply because we do not know why they should not come equally often! Much more likely is it that girls are born because boys have

been born, and conversely. Is there any sense to an approximately equal ratio unless there are equilibrating forces?

Recently Coe has published an illuminating discussion of sexual development.[15] His thesis is that the organism makes a choice of its sex early in its life, and may occasionally change its mind later, either successfully or with resultant hermaphroditism. Obviously the secret of the sex ratio must lie in the conditions that determine this 'choice.' For human beings we do not know why the birth of many girls should favor the birth of more boys, yet that must be a fact if it is also a fact that the sex ratio tends toward constancy.[16] Coe cites examples of how the ratio may be maintained in some invertebrates. The snail Crepidula passes through a male stage to become finally a female. If no females are present, the process is rapid, so that there are soon some females with the males. With females present the process is slower. There is equilibration. The determinants which slow the feminizing process of maturation are "the stimuli which the male receives through his sense organs and the associated internal changes occurring at the time of mating," not presumably any transfer of hormonal masculinizing substance. A similar determination of the sex ratio happens with the worm Bonellia. The annelid Ophrytrocha, which also develops through a male stage to the female, may, under unfavorable conditions like starvation, revert from the female stage to the male. It has been noted that when two females are in isolation, the more dominant female may devour the food, thus forcing her companion back into a male stage in which he is able to fertilize her eggs. Would anyone want in these cases to say that the sex ratio is determined by chance, that we must learn to

[15] W. R. Coe; Divergent pathways in sexual development, *Science*, 1940, 91, 175–182, esp. 177f.

[16] von Mises, *op. cit.*, 198–202, discusses Karl Marbe's problem: Does a man, about to become a father and wishing for a son, have more chance of getting his desire if he finds that the 17 immediately preceding births, registered in his town, were girls, in view of the fact that in 49,152 registrations there has been no instance of 18 or more iterations of registration of the same sex? Von Mises concludes that the chances of a son are still only 50–50 for the expectant father. My question is somewhat different. What are the father's chances of a son (*a*) *if* the sex ratio tends to be equal but (*b*) *if* at the time of the birth it is very unequal? Not 50–50.

think in terms of indeterminacy, and that it is only the inertia of thought that makes us think that a knowledge of causal determinants is better than a statement of statistical frequencies?

VI. Conclusions

This article is a demand for more energetic discontent when the determinants of the limiting frequencies of convergent statistical series remain unknown. The Principle of Indifference, when it is used to predict frequencies in the absence of observation, is only ignorance masquerading as knowledge. It is, moreover, insidious because it falsely satisfies the discontent that ought to go with ignorance, so that it stops the search for the determinants. Even when the frequency theory of probability has been accepted, the Principle of Indifference may slip in, so habituated to it is thought. The Principle of Independence, appearing perhaps as the Law of Large Numbers or in some other form that works for indeterminacy, is just as great an enemy of research into the causal ground of frequencies as is the Principle of Indifference, but it is a mischief that is not at all generally recognized. With the Heisenberg principle itself one cannot quarrel: When we must be ignorant, we must be ignorant. Let us not encourage ourselves, however, into any patient acceptance of any ignorance that can be dispelled. The use of statistical indeterminacy in most macroscopic science is an unnecessary and premature denial of the effectiveness of determinism.

[MS. received October 24, 1940]

THE HORMONAL INDUCTION OF BEHAVIOR

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I. Introduction

As a reaction against the mentalism of an earlier period, psychology in the past half-century has increasingly focused on the observable facts of behavior. Since events taking place in the brain are difficult to observe, psychologists turn to the receptors and effectors for the explanation of their data. The effect of this tendency is to reduce the brain to a switchboard. Mental events which in any way transcend the immediate stimuli are referred to motor reactions and their proprioceptive consequents. Thus thinking (**76**, p. 322 ff.), feeling (**16**, p. 410 ff.), attention (**83**, p. 192),[1] purpose (**35**), judgment (**75**), and consciousness itself (**38, 48**) have at one time or another been held dependent on reactions of the smooth or striped musculature. The study of postural tensions is to provide the key to many problems of dynamics and psychopathology (**27**).

On the whole this centrifugal shift has proved stimulating to research and has some significant discoveries to its credit, such as the role of stomach contractions in hunger (**19**) and of muscular innervation in thought (**49**). In certain directions the results have failed to support the response theory, as in the attempts to verify the James-Lange theory of emotions (**12**) or the eye-movement theory of optical illusions (**82**, p. 648). In such cases one value of the research, as Bard has pointed out, is to redirect attention to the central nervous system.

In attempting to understand the mechanism of reproductive behavior we encounter this problem in a special form. Since reproductive activities are known to depend on endo-

[1] But see Mowrer *et al* (**52**) for an objective demonstration of central factors.

crine secretions, the question arises as to how these agents exert their influence. According to the peripheral viewpoint the organic drives are determined by conditions in the tissues controlled by the autonomic system. Since certain of these tissues are directly susceptible to gonadal hormones, they may provide the basis of the sexual and maternal drives. On the other hand, it is possible that the hormones act directly on the central nervous system in such a way as to 'lubricate' the appropriate mechanisms. The problem of the present paper, then, may be stated as follows: *In the hormonal induction of behavior are the nerve centers themselves chemically altered or do they merely transmit afferent impulses from hormone-sensitive tissues?* [2]

Although experimenters in this field are more prone to accept the central view (**25, 39, 57, 70**), the writers of general discussions are likely either to pass over this question or to adopt the peripheral viewpoint. Dashiell, for example, in describing Wang's and Richter's experiments on the relation of the gonads to spontaneous activity, writes of the sex drive as follows:

> This organic urge arises from a condition in the sex apparatus. . . . Thus it was proved that the hypertonicity and hyperactivity of the animal was excited by stimulations of intraorganic origin (in the sex apparatus) when it was in certain physiological conditions. . . . The excess activity . . . is set up as the result of the *afferent neural currents* arising in the sex apparatus . . . (**24**, p. 105 f. Italics ours).

Klein incorporates a peripheral mechanism in his definition of sex urge:

> By the tensions of sex we mean those *organic sensations* which prompt the animal to indulge in behavior biologically necessary for the reproduction of its kind (**37**, p. 153. Italics ours).

[2] There are actually three possible loci of hormone action: the peripheral structures, the autonomic ganglia, and the gray matter of the cerebrospinal system. Since impulses to the autonomic ganglia can induce movements of the skeletal muscles only by reflection from effectors, the experimental data do not permit a distinction between ganglia and viscera. In the following discussion, then, the term 'central nervous system' actually refers to the cerebrospinal system as distinguished from the autonomic-visceral complex.

After presenting arguments against what he terms the *glandular tension* theory (distension of the male glands with fluid), he proposes a counter-theory according to which

> the primary physiological goad to sex behavior is a change in the *tonus* of the smooth muscles connected with the reproductive apparatus (**37**, p. 156).

Since 'in the interests of simplicity of exposition' the author has 'deliberately refrained from considering endocrinal influences' (**37**, p. 157), he implies that these influences are only indirectly concerned in the sex drive.

Lund, without explicitly recognizing the distinction, *implies* both a direct (central) and an indirect (peripheral) hormone action in the following quotation:

> But while there seems to be no question about this *general* dependence of sexual desire upon endocrine secretion, there appear to be certain non-endocrine factors which noticeably affect changes in susceptibility. Thus in the male, periodic variations in sex desire are closely related to the filling and distention of the seminal vesicles (**43**, p. 148).

Young (**83**) cites Lashley (**40**) as favoring a theory of direct endocrine influence on sexual reflex centers, but in his own discussion seems to abandon this notion in favor of a peripheral, or 'mechanical,' theory. Thus:

> All fundamental drives have certain common characteristics, namely: (*a*) a persistent condition in the tissues; which gives rise to (*b*) a sustained stimulation of afferent nerves; from the latter (*c*) a release of energy in nerves, muscles, and other tissues . . . (p. 137).

This position is not held unreservedly, however, for although

> some drives [food hunger . . . the appetite for suckling in the lactating mother . . . and to some extent . . . the male sexual drive] depend upon a mechanical pressure on receptors located in the walls of hollow viscera, . . . still other drives [the female sex urge to some extent, and probably certain cravings or aversions for specific food substances] depend upon some internal chemical factor. . . . The chemical agents in the blood stream are commonly assumed to excite the nerve centers directly (p. 137 f.).

With a final quotation from Holt (**33**), an advocate of the pe-

ripheral theory, Young leaves the reader with the conclusion, aptly expressed in the author's own words: "How the hormones act to establish a specific drive is not known in detail" (p. 138).

Lashley (**39**) has presented the most thorough analysis of this problem. In a critical review of research he minimizes alternative hypotheses and reaffirms his earlier position:

> There remains only the last alternative, that the hormones act upon the central nervous system to increase the excitability of the sensorimotor mechanism specifically involved in the instinctive activity (p. 462).

Bard (**11**) has also discussed the problem briefly but declined to draw a general conclusion. We shall here examine the experimental evidence in more detail.[3]

II. Evidence

Spontaneous activity.—In Lashley's discussion (**39**) we may discern two distinct aspects of the problem of hormonal sensitization: (1) locus, central or peripheral; (2) distribution, specific or general. With respect to the second question, his point that the induction of mating behavior cannot be explained by increased general excitability seems well taken, since there is no evidence that castration, for example, reduces responsiveness to stimuli in general as much as to sexual stimuli. Hunt and Schlosberg (**36**) report a 9 per cent reduction of general activity in castrated male rats during a period in which about 50 per cent of male rats cease to copulate (**72**). Seward and Papanicolaou (unpublished) found no effect of castration or testosterone propionate on the activity of male guinea pigs, although responsiveness to females fell and rose again (**64**). The appearance, moreover, of a distinct pattern of behavior in the œstrous female argues against a mere increase of excitability.

But what of the well-known effect of the gonads on spontaneous activity as measured by the drum-running of rats

[3] The present discussion is limited to results on mammals. Since controlled research on the endocrine factors in sex behavior has until recently been confined to subprimate forms, the evidence is mainly derived from rodents and carnivores.

(21, 30, 56, 58, 61)? We may assume that *besides* their specific effect the gonads induce a general restlessness which is released by rotating the drum. Or we may consider running the expression of an intensified sex drive in the absence of the adequate stimulus. If the latter were true, sexual satiation should diminish the activity. Little evidence is available on this point. Richter (57) reported that in male rats as many as 30 copulations had no effect on the next day's activity level. Ball (7), studying the effect of œstrogenic hormone on castrated males, found some lowering of activity on the test nights; *i.e.*, following a half-hour with receptive females. However, the difference was not sufficiently great to suggest that the sex drive itself is more than a contributory factor in running.

Turning to the problem of where the hormones exert their effect, we shall proceed on the assumption that the gonads are directly responsible.[4] We must then ask whether their influence is exerted on visceral-autonomic structures or on the central nervous system. Several lines of evidence indicate that drum-running is independent of impulses from the internal genital organs. It is not affected by removal of the uterus or seminal vesicles (57). Œstrin restored the activity level of spayed rats independently, in some cases, of its effect on the genital tract (57). This was confirmed by Richter and Hartman (61), who found that amniotin had a clear-cut, positive effect on the activity of males without preserving their sex organs from atrophy. Bacq (4) reported that after abdominal sympathectomy [5] in females the œstrous peaks of activity persisted, while in males the postoperative enlargement of the seminal vesicles did not alter their activity level.

Mating behavior.—The dependence of mating responses on the sex hormones has been thoroughly established by the results of gonadectomy and of implants and injections (17, 20,

[4] Since Richter has demonstrated that the hypophysis (62), adrenals (60), and thyroids (59) are concerned in the regulation of spontaneous activity, it is possible that the gonads exert their influence by disturbing the endocrine balance. The œstrous peaks of activity in the normal female, however, argue against the more circuitous explanations.

[5] Presumably denervating the internal and partially the external generative organs.

21, 25, 30, 46, 50, 64, 67, 71, 77, 80). That this relationship exists independently of the reproductive organs is also strongly indicated:

Female rats from which the uterus and vagina had been removed showed normal sex behavior (8). A rat (5) and two guinea pigs (53) deprived of ovaries and uterus came into heat when injected with œstrin followed by progesterone. One of the guinea pigs was subjected to a further operation in which the vagina was completely removed (Papanicolaou, Seward and Seward, unpublished).[6] After injection with œstrin-progesterone, stimulation by a male or by finger-stroking elicited copulatory responses of normal intensity.

Abdominal sympathectomy had no reported effect on mating reactions in the female rat (3). More drastic procedures were carried out on cats (10) and rabbits (18). Female cats, normal and ovariectomized, were subjected to removal of uterus and proximal vagina, abdominal sympathectomy, or sacral cord removal, or to combinations of two or all three operations. Treatment with œstrogenic substances induced the complete unmodified pattern of œstrous behavior, with the exception that the 'after-reaction,' which normally follows stimulation of the distal vagina, was abolished by sacral cord removal. In rabbits the same three operations did not prevent mating.

Similar results have been obtained in the male. Steinach[7] found that removal of the seminal vesicles and prostate gland had no effect on sex behavior (47, p. 234 ff.). The same is true of vasectomy (66) and removal of the vasa deferentia (6). After abdominal sympathectomy the sex behavior of rodents was normal except for the failure to ejaculate (2). Root and Bard (63), working with cats, denervated the entire genital region by removal of the sacral and lower two lumbar segments of the cord coupled with abdominal sympathectomy.

[6] Film shown at the 47th annual meeting of the American Psychological Association, Stanford University, 1939.

[7] E. Steinach, Untersuchungen zur Vergleichenden Physiologie der Männlichen Geschlechtsorgane insbesondere der accessorischen Geschlechtsdrüsen. *Pflüger's Arch. f. d. ges. Physiol.*, 1894, 56, 304–338. The original source was consulted too late for inclusion in the bibliography.

These males, although unable to achieve erection or intromission, showed 'no diminution in sexual aggressiveness.'

Experiments on the artificial reversal of sex behavior, though stressed by Lashley (**39**), seem less conclusive, considered by themselves, than some of the foregoing studies. This is because both the male and female sex hormones induce changes in the organs of the opposite sex. Male hormone causes hypertrophy of the clitoris (**9, 34**), while œstrin modifies the prostate and seminal vesicles (**1, 79**). The effect of these changes on afferent impulses from this region is unknown. As significant as the reported cases of behavior reversal appears the finding of Ball (**7**) that œstrin injected into male rats castrated after maturity *reactivated the complete masculine behavior pattern.* If this pattern were dependent on impulses from the genital organs, some modification would be expected.

This and other results (**5, 86**), together with the reversals of mating pattern which occur in normal animals (**14, 85**), indicate that masculine and feminine behavior are not altogether specific in their modes of arousal but have different thresholds in the two sexes. It is this, finally, which suggests that experimental reversals depend on the lowering of response thresholds rather than on the changing of internal stimulus conditions.

Maternal behavior.—The well-known drainage theory, that the various activities centering around the care of the young are subsidiary to the emptying of the mammary glands, is based on the close relationship normally found between maternal care and lactation. A number of experiments, however, as already reviewed (**65**), have shown that this relationship is not a necessary one. Not only has lactation been produced without a disposition to nurse, but maternal care has been observed in rats, mice and other forms in the absence of lactation. What, then, is the essential mechanism? The retrieving of young by virgin rats (**78**) and by prepubertal and hypophysectomized male and female mice (**41, 42**), as well as its prolongation by substituting newborn litters (**74, 78**), emphasizes the importance of the stimulus object. Yet all the

observers cited furnish evidence of the significance of the animal's internal condition. Injection experiments (**65**, p. 22) indicate that care of the young and lactation, though separable, are both under hormonal influence. It is surely an oversimplification to lump all maternal activities. However, the facts at present suggest the concept of a central mechanism activated partly by secretions of the hypophysis or gonads or both and partly by an external stimulus pattern.

III. Evaluation

The foregoing evidence indicates that in a number of mammals the typical reproductive activities can occur without reinforcement from the viscera most concerned.[8] It suggests that the hormones may affect behavior by a direct action on the central nervous system. This should not be taken to imply that autonomic reflexes play no part in mating behavior. Aside from the orgasm, there is little doubt that they contribute to the preceding summation of excitement. They may be responsible for the intensification of drive sometimes observed just after copulation (**64**, **73**). In so far as these reflexes are altered by hormonal action on their sense-organs and effectors a supplementary peripheral mechanism must be recognized.[9] We have seen, however, that an abdominal component is not *essential* to the initiation and much of the execution of the mating pattern under hormonal influence.

But the elimination of the 'sexual apparatus' does not compel the adoption of a central hypothesis. Obviously all other possible sites of hormone action have not been excluded. One such possibility may be mentioned here: *i.e.*, the existence of hormone-sensitive receptors in the membranes of the mouth or nose. The recognized effects of sex hormones on the nasal tissues (**28**, **51**) give the suggestion greater plausibility. Fur-

[8] The argument that sex responses after deafferentation may be due to previous conditioning is discounted by the failure of experience to preserve sex behavior in the castrated male (**72**) or the dioestrous female.

[9] A further quantitative effect of autonomic factors on cerebrospinal reactions was demonstrated by Herren and Haterius (**31**, **32**). They found a difference in Achilles reflex time of the rat between œstrus and dioestrus due to the follicular and luteal hormones. This difference was abolished after abdominal sympathectomy.

ther experiments should combine denervation of this area with that of the genital region. One female rabbit, however, in which complete genital denervation was followed by removal of the olfactory bulbs (**18**) not only mated but "on one occasion mounted another female and reached a peak of excitement which resulted in ovulation" (p. 550).

A somewhat analogous situation is presented by hunger and thirst. In the case of hunger peripheral theory, in the form of stomach contractions, is supported by experimental evidence (**19**). There are many findings, however, which this theory does not explain:[10] *e.g.*, the survival of a hunger drive in rats after removal of the stomach (Tsang) or denervation of the gastric region (Bash), the compensatory shifts of appetite in pregnancy and lactation, absence of adrenals, and vitamin-B deficiency (Richter and associates), and the hoarding behavior of non-hungry rats on an inadequate diet (**81**). Instead of adopting a central theory, however, Young (**84**) has proposed that the chemo-receptors of taste and smell may be altered by blood-stream conditions and thus control food behavior, a theory which receives some confirmation from Richter's finding of a lowered taste threshold for salt in adrenalectomized rats. In the case of thirst the peripheral theory which attributes the drive to receptors in the pharynx (**19**) fails to explain the unchanged drinking by dogs after division of the sensory tracts from this area (Bellows and Van Wagenen). In both cases the hypothesis of humoral regulation of brain centers lurks in the background.

In the study of how blood-stream conditions modify behavior, theories which localize the process in specific receptors are preferable because the receptors can be excluded and the effect on behavior observed. A crucial test of the central theory would require the isolation of the center from *all* plausible afferent sources or the restriction of the blood-stream condition to the center itself. Either procedure would make a complex behavior test impossible. Yet in view of the weight of evidence it may not be premature to suggest that in

[10] For complete documentation of this paragraph see the recent review by Marquis (**45**).

one aspect of reproductive behavior a crucial test of the central theory is not inconceivable.

In the last few years attempts have been made to locate a 'sexual center' in the brain (**11**). A center for male behavior, with its relative complexity of sensory and motor organization, has not been found. Severe invasions of the neocortex reduced the frequency of copulation in rats (**13**), and when its complete removal was combined with ablation of the olfactory bulbs mating in rabbits was abolished (**18**). The results indicated, however, that the deficiency was in the arousal, rather than the execution, of the response, suggesting that the motor pattern is subcortically integrated. The organization of maternal behavior is even more obscure (**15**, **69**). In the female cat and guinea pig, however, œstrous reactions have been hormonally induced after ablation of all brain tissue above the superior colliculi and mammillary bodies but abolished by intercollicular decerebration (**26**). Since the weight of evidence is against the hormonal induction of spinal reflexes in these forms,[11] the hormone-sensitive tissue may be restricted to a small subcortical area.

The copulatory reflex of the female guinea pig is a clear-cut response which can be elicited in the spayed animal, after hormone treatment, by manual stimulation. The same is true of parts of the 'courtship behavior' of the female cat (**10**). By using a technique modeled on that of the 'isolated head,' as applied by Heymans to the problem of the nervous regulation of the heart (**68**, p. 799) and by La Barre to that of the stomach (**55**, p. 256), it might be possible to test for 'heat' after supplying hormone-treated blood to the head alone. If, after gustatory and olfactory denervation, such tests were positive, the central theory would be conclusively proved.

Even then, it is true, we should still be far from an understanding of the postulated humoro-neural process. Lashley (**39**) has pointed out the need for positive evidence of a selective chemical action on particular nerve centers. Although evidence is accumulating of differential thresholds to drugs at

[11] Some controversy exists on this point (**44**; **11**, p. 557 ff.).

different levels (**22**, **23**, **29**), the classical example of the 'humoro-neural reflex' (**54**) is still the stimulation of the respiratory center by an increase of CO_2 in the blood. It may be pointed out that the details of the chemical regulation of behavior, once the process has been localized, should eventually yield to physiological techniques, whether applied to peripheral receptors or to central neurones.

IV. Summary

Modern psychology tends to focus on the two ends of the response arc to the neglect of the central portion. In the field of reproductive activities the theory that hormones exert their effects on behavior through the genital tract is examined in the light of experimental evidence. This theory has been proved inadequate. The evidence strongly suggests a direct hormonal action on the brainstem, but the crucial experiment has yet to be performed.

Bibliography

1. Allen, E., Hisaw, F. L., & Gardner, W. U. The endocrine functions of the ovaries. In *Sex and internal secretions* (Ed., E. Allen), Baltimore: Williams & Wilkins, 1939, pp. 452–629.
2. Bacq, Z. M. Impotence of the male rodent after sympathetic denervation of the genital organs. *Amer. J. Physiol.*, 1931, **96**, 321–330.
3. ——. The effect of sympathectomy on sexual functions, lactation, and the maternal behavior of the albino rat. *Amer. J. Physiol.*, 1932, **99**, 444–453.
4. ——. The effects of abdominal sympathectomy, adrenal inactivation and removal of the stellate ganglia on the spontaneous activity of the albino rat. *Endocrinol.*, 1931, **15**, 34–40.
5. Ball, J. Male and female mating behavior in prepubertally castrated male rats receiving estrogens. *J. comp. Psychol.*, 1939, **28**, 273–284.
6. ——. Normal sex behavior in the rat after total extirpation of the vasa deferentia. *Anat. Rec.*, 1934, **58**, 49.
7. ——. Sex activity of castrated male rats increased by estrin administration. *J. comp. Psychol.*, 1937, **24**, 135–144.
8. ——. Sex behavior of the rat after removal of the uterus and vagina. *J. comp. Psychol.*, 1934, **18**, 419–422.
9. ——. The effect of testosterone on the sex behavior of female rats. *J. comp. Psychol.*, 1940, **29**, 151–165.
10. Bard, P. The effects of denervation of the genitalia on the œstrual behavior of cats. *Amer. J. Physiol.*, 1935, **113**, 5–6.
11. ——. The hypothalamus and sexual behavior. *Res. Publ. Ass. Nerv. Ment. Dis.*, 1940, **20**, 551–579.
12. ——. The neuro-humoral basis of emotional reactions. In *A handbook of general experimental psychology* (Ed., C. Murchison), Worcester, Mass.: Clark Univ. Press, 1934. Pp. 264–311.

13. BEACH, F. A. Effects of cortical lesions upon the copulatory behavior of male rats. *J. comp. Psychol.*, 1940, **29**, 193–244.
14. ——. Sex reversals in the mating pattern of the rat. *J. genet. Psychol.*, 1938, **53**, 329–334.
15. ——. The neural basis of innate behavior. I. Effects of cortical lesion upon the maternal behavior pattern in the rat. *J. comp. Psychol.*, 1937, **24**, 393–440.
16. BEEBE-CENTER, J. G. *The psychology of pleasantness and unpleasantness*, New York: Van Nostrand, 1932. P. 427.
17. BOLING, J. L., & BLANDAU, R. J. The estrogen-progesterone induction of mating responses in the spayed female rat. *Endocrinol.*, 1939, **25**, 359–364.
18. BROOKS, C. M. The role of the cerebral cortex and of various sense organs in the excitation and execution of mating activity in the rabbit. *Amer. J. Physiol.*, 1937, **120**, 544–553.
19. CANNON, W. B. Hunger and thirst. In *A handbook of general experimental psychology* (Ed., C. Murchison), Worcester, Mass: Clark Univ. Press, 1934. Pp. 247–263.
20. COLLINS, V. J., BOLING, J. L., DEMPSEY, E. W., & YOUNG, W. C. Quantitative studies of experimentally induced sexual receptivity in the spayed guinea-pig. *Endocrinol.*, 1938, **23**, 188–196.
21. COMMINS, W. D., & STONE, C. P. Effects of castration on the behavior of mammals. *Psychol. Bull.*, 1932, **29**, 493–508.
22. CULLER, E., COAKLEY, J. D., SHURRAGER, P. S., & ADES, H. W. Differential effects of curare upon higher and lower levels of the central nervous system. *Amer. J. Psychol.*, 1939, **52**, 266–273.
23. CURRAN, F. J. The effects of barbiturates and bromides on mental and emotional processes. *Res. Publ. Ass. Nerv. Ment. Dis.*, 1939, **19**, 154–171.
24. DASHIELL, J. F. *Fundamentals of general psychology*, Boston: Houghton Mifflin, 1937. P. 655.
25. DEMPSEY, E. W., HERTZ, R., & YOUNG, W. C. The experimental induction of œstrus (sexual receptivity) in the normal and ovariectomized guinea pig. *Amer. J. Physiol.*, 1936, **116**, 201–209.
26. ——., & RIOCH, D. McK. The localization in the brain stem of the œstrous responses of the female guinea pig. *J. Neurophysiol.*, 1939, **2**, 9–18.
27. FREEMAN, G. L. The postural substrate, PSYCHOL. REV., 1938, **45**, 324–334.
28. HAMILTON, J. B. Changes in nasal mucosa of monkeys (Macaca rhesi) and humans by male hormone substances. *Proc. Soc. Exper. Biol. & Med.*, 1937, **37**, 366–369.
29. HEINBECKER, P., & BARTLEY, H. Manner of strychnine action on nervous system. *Amer. J. Physiol.*, 1939, **125**, 172–187.
30. HEMMINGSEN, A. M. Studies on the œstrus-producing hormone (œstrin). *Skand. Arch. f. Physiol.*, 1933, **65**, 97–250.
31. HERREN, R. Y., & HATERIUS, H. O. On the mechanism of certain ovarian hormonal influences on the central nervous system. *Amer. J. Physiol.*, 1932, **100**, 533–536.
32. ————. The relation of ovarian hormones to electro-myographically determined Achilles reflex time. *Amer. J. Physiol.*, 1931, **96**, 214–220.
33. HOLT, E. B. *Animal drive and the learning process.* New York: Holt, 1931. P. 307.
34. HU, C. K., & FRAZIER, C. N. Masculinizing of adult female rabbit following injection of testosterone propionate. *Proc. Soc. Exper. Biol. N. Y.*, 1939, **42**, 820–823.
35. HULL, C. L. Goal attraction and directing ideas conceived as habit phenomena. PSYCHOL. REV., 1931, **38**, 487–506.
36. HUNT, J. McV., & SCHLOSBERG, H. The influence of illumination upon general activity in normal, blinded, and castrated male white rats. *J. comp. Psychol.*, 1939, **28**, 285–298.

37. KLEIN, D. B. *General psychology.* New York: Holt, 1936. P. 560.
38. LANGFELD, H. S. A response interpretation of consciousness. PSYCHOL. REV., 1931, **38**, 87–108.
39. LASHLEY, K. S. Experimental analysis of instinctive behavior. PSYCHOL. REV., 1938, **45**, 445–471.
40. ——. Physiological analysis of the libido, PSYCHOL. REV., 1924, **31**, 192–202.
41. LEBLOND, C. P. Extra-hormonal factors in maternal behavior. *Proc. Soc. Exper. Biol. N. Y.*, 1938, **38**, 66–70.
42. ——, & NELSON, W. O. Maternal behavior in hypophysectomized male and female mice. *Amer. J. Physiol.*, 1937, **120**, 167–172.
43. LUND, F. H. *Emotions.* New York: Ronald Press, 1939. P. 305.
44. MAES, J. P. Neural mechanism of sexual behavior in the female cat. *Nature*, London, 1939, **144**, 598–599.
45. MARQUIS, D. G. Physiological psychology. *Ann. Rev. Physiol.*, 1940, **2**, 433–461.
46. MARRIAN, G. F., & PARKES, A. S. The relative amounts of œstrin required to produce the various phenomena of œstrus. *J. Physiol.*, 1930, **69**, 372–376.
47. MARSHALL, F. H. A. *The physiology of reproduction*, London: Longmans, 1910. P. 706.
48. MAX, L. W. An experimental study of the motor theory of consciousness, I. Critique of earlier studies. *J. gen. Psychol.*, 1934, **11**, 112–125.
49. ——. Experimental study of the motor theory of consciousness, IV. Action-current responses in the deaf during awakening, kinesthetic imagery and abstract thinking. *J. comp. Psychol.*, 1937, **24**, 301–344.
50. MOORE, C. R., & PRICE, D. Some effects of testosterone propionate in the rat. *Anat. Rec.*, 1938, **71**, 59–78.
51. MORTIMER, H., WRIGHT, R. P., & COLLIP, J. B. The effect of the administration of œstrogenic hormones on the nasal mucosa of the monkey (Macaca mulatta). *Canad. Med. Assoc. J.*, 1936, **35**, 503–513.
52. MOWRER, O. H., RAYMAN, N. N., & BLISS, E. L. Preparatory set (expectancy)—an experimental demonstration of its central locus. *J. exper. Psychol.*, 1940, **26**, 357–372.
53. PAPANICOLAOU, G. N., & SEWARD, J. P. Artificial œstrus in the guinea pig without ovaries and uterus. *Psychol. Bull.*, 1939, **36**, 535 (Abstract).
54. PARKER, G. H. Novel types of nerve reflexes. *Proc. Nat. Acad. Sci., Wash.*, 1940, **26**, 246–249.
55. PATTERSON, T. L. Comparative physiology of the gastric hunger mechanism. *Ann. N. Y. Acad. Sci.*, 1933, **34**, 55–272.
56. PEDERSEN-BJERGAARD, K., & MADSEN, G. B. The effect of œstrogenic and androgenic hormone on the spontaneous muscular activity of gonadectomized male and female rats. *Acta Path. et Microbiol. Scand.*, Suppl. 37, 1938, **15**, 431–437.
57. RICHTER, C. P. Animal behavior and internal drives. *Quart. Rev. Biol.*, 1927, **2**, 307–343.
58. ——. The effect of early gonadectomy on the gross body activity of rats. *Endocrinol.*, 1933, **17**, 445–450.
59. ——. The role played by the thyroid gland in the production of gross body activity. *Endocrinol.*, 1933, **17**, 73–87.
60. ——. The spontaneous activity of adrenalectomized rats treated with replacement and other therapy. *Endocrinol.*, 1936, **20**, 657–666.
61. ——, & HARTMAN, C. G. The effect of injection of amniotin on the spontaneous activity of gonadectomized rats. *Amer. J. Physiol.*, 1934, **108**, 136–143.
62. ——, & WISLOCKI, G. B. Anatomical and behavior changes produced in the rat by complete and partial extirpation of the pituitary gland. *Amer. J. Physiol.*, 1930, **95**, 481–492.
63. ROOT, W. S., & BARD, P. Erection in the cat following removal of lumbo-sacral segments. *Amer. J. Physiol.*, 1937, **119**, 392–393.

64. SEWARD, J. P. Studies on the reproductive activities of the guinea pig. III. The effect of androgenic hormone on sex drive in males and females. *J. comp. Psychol.*, 1940, **30**, 435–449.
65. ——, & SEWARD, G. H. Studies on the reproductive activities of the guinea pig. I. Factors in maternal behavior. *J. comp. Psychol.*, 1940, **29**, 1–24.
66. SLONAKER, J. R. The effect of the excision of different sexual organs on the development, growth and longevity of the albino rat. *Amer. J. Physiol.*, 1930, **93**, 307–317.
67. SOLLENBERGER, R. T., & HAMILTON, J. B. The effect of testosterone propionate upon the sexual behavior of castrated male guinea pigs. *J. comp. Psychol.*, 1939, **28**, 81–92.
68. *Starling's Principles of human physiology*, 6th. ed. (Ed., C. L. Evans), Philadelphia: Lea and Febiger, 1933. P. 1122.
69. STONE, C. P. Effects of cortical destruction on reproductive behavior and maze learning in albino rats. *J. comp. Psychol.*, 1938, **26**, 217–236.
70. ——. Experimental studies of two important factors underlying masculine sex behavior: the nervous system and the internal secretion of the testis. *J. exper. Psychol.*, 1923, **6**, 84–106.
71. ——. Physiological psychology. Part II, *Ann. Rev. Physiol.*, 1939, **1**, 487–502.
72. ——. The retention of copulatory ability in male rats following castration. *J. comp. Psychol.*, 1927, **7**, 369–387.
73. ——, BARKER, R. G., & TOMILIN, M. I. Sexual drive in potent and impotent male rats as measured by the Columbia obstruction apparatus. *J. genet. Psychol.*, 1935, **47**, 33–48.
74. STURMAN-HULBE, M., & STONE, C. P. Maternal behavior in the albino rat. *J. comp. Psychol.*, 1929, **9**, 203–237.
75. WASHBURN, M. F. A system of motor psychology. In *Psychologies of 1930* (Ed., C. Murchison), Worcester, Mass.: Clark Univ. Press, 1930. Pp. 81–94.
76. WATSON, J. B. *Psychology from the standpoint of a behaviorist.* Philadelphia: Lippincott, 1919. P. 429.
77. WIESNER, B. P., & MIRSKAIA, L. On the endocrine basis of mating in the mouse. *J. exper. Physiol.*, 1930, **20**, 273–279.
78. ——, & SHEARD, N. M. *Maternal behaviour in the rat.* London: Oliver & Boyd, 1933. P. 245.
79. WITSCHI, E. Modification of the development of sex in lower vertebrates and in mammals. In *Sex and internal secretions* (Ed., E. Allen), Baltimore: Williams & Wilkins, 1939. Pp. 145–226.
80. ——, & PFEIFFER, C. A. The hormonal control of œstrus, ovulation and mating in the female rat. *Anat. Rec.*, 1935, **64**, 85–105.
81. WOLFE, J. B. An exploratory study of food-storing in rats. *J. comp. Psychol.*, 1939, **28**, 97–108.
82. Woodworth, R. S. *Experimental psychology.* New York: Holt, 1938. P. 889.
83. YOUNG, P. T. *Motivation of behavior.* New York: Wiley, 1936. P. 562.
84. ——. Preferences and demands of the white rat for food. *J. comp. Psychol.*, 1938, **26**, 545–589.
85. YOUNG, W. C., DEMPSEY, E. W., HAGQUIST, C. W., & BOLING, J. L. Sexual behavior and sexual receptivity in the female guinea pig. *J. comp. Psychol.*, 1939, **27**, 49–68.
86. ——, & RUNDLETT, B. The hormonal induction of homosexual behavior in the spayed female guinea pig. *Psychosom. Med.*, 1939, **1**, 449–460.

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NEGATIVE PRACTICE AND THEORIES OF LEARNING[1]

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I. Introduction

In 1928 Dunlap published a short paper in *Science* (3) in which he reported his now familiar observation that errors could best be corrected by practicing the errors themselves with knowledge of their incorrectness. This procedure, which has been designated 'negative practice' in contrast to so-called 'positive practice' where the correct response is rehearsed, was applied to the correction of an occasional tendency to type *h-t-e* instead of *t-h-e*, and Dunlap reported that the *h-t-e* habit was eliminated with a promptness which contrasted sharply with the ineffectiveness of having written *t-h-e* more frequently than *h-t-e* for many years. On these grounds the observations were offered as evidence against a frequency theory of learning.

A number of other negative practice experiments, which are summarized elsewhere (17), have sought to determine the relative effectiveness of positive and negative practice in many different behavior situations such as nail-biting, typing, tics, piano playing, and have contributed results which may be condensed into the following statements. (1) In some settings negative practice brings about the elimination of a wrong response and the establishment of a correct one, a fact which suggests that a response may be reinforced without being repeated. (2) Sometimes negative practice is superior to and again it may equal or be inferior to positive practice. (3) In some situations repetition eliminates the repeated response (the wrong response in negative practice), in others, it establishes the repeated response (the right response in positive

[1] I am greatly indebted to Dr. D. G. Marquis of Yale University for a critical reading of the manuscript and for a number of valuable suggestions.

practice). In other words, the effects of repetition are inconsistent.

Although Dunlap was not the first to offer such experimental evidence against a frequency principle of learning, he was undoubtedly responsible for much of the exploitation of the negative practice situation to this end and many of the interpretations of the evidence have followed his lead.[2] In the early papers on negative practice his contribution to interpretation consisted of the proposal that there were three alternative statements about the effects of repetition on learning which were tested in some degree by the negative practice experiments. His well known *alpha*, *gamma*, and *beta* hypotheses took the following form: *alpha*, "A response to a given stimulus pattern definitely increases the probability that on the recurrence of the same, or substantially the same stimulus pattern, the same, or substantially the same response will occur"; *beta*, "The response in itself has no effect on the future probability of the same stimulus pattern producing the same response"; and *gamma*, "The response decreases the probability"[3] (3). It was pointed out that the observations on negative practice might be taken to confirm either *gamma* or *beta* but that the latter alternative was preferred for reasons of parsimony.

In similar fashion Poindexter reported a negative practice experiment designed to test the *alpha*, *gamma*, and *beta* hypotheses. Having found positive practice superior, he con-

[2] It happens that most of the experiments which have grown out of Dunlap's studies have involved situations where it was possible for each subject to devote all his practice trials exclusively to repeating either the wrong or the right response, as for example, in the practice of a typed word. There are situations like the maze, however, where this is impossible because the correct pattern of reactions must be made at least once on each successful trial in order that the goal be reached and the trial completed. Since alternate practice of right and wrong responses must complicate matters considerably, the present discussion will be confined chiefly to those experiments in which one or the other type of practice has been carried through exclusively, although it is obvious that many of the same factors must be involved in both situations, since practice of the right and wrong responses in the maze has often been shown to be superior to practice of right responses alone (1, 12, 13).

[3] These same definitions were given in Dunlap's 1930 article (4) but for some unexplained reason the definitions of *gamma* and *beta* were interchanged in 1932 (5). At this time the *gamma* hypothesis asserted that repetition had no effect. Since most writers seem to have adopted the usage first proposed, that one is employed in this paper.

cluded that the *alpha* hypothesis was confirmed (**19**). Kellogg and White found negative practice superior in learning a stylus maze under certain circumstances and concluded that the *beta* hypothesis was thereby supported (**11**).[4] Mowrer has also included Dunlap's work on negative practice in a list of experiments which ". . . seem to point to the conclusion that the mere frequency with which a given response is elicited by an unconditioned stimulus, is not related in any very direct or important way to the establishment or persistence of new stimulus response relationships" (**15**, p. 63), and Tolman has likewise cited the negative practice experiments as part of the evidence that, "Repetition in the sense of one of the alternative responses as against any of the others as the cause of learning plays little if any role" (**21**, p. 362). This again amounts to a statement that these experiments confirm the *beta* hypothesis. Such illustrations are sufficient to indicate the general acceptance of negative practice data as relevant to the proof or disproof of Dunlap's hypotheses.

It is the task of the present paper to evaluate this sort of evidence on the role of frequency in learning and to offer a discussion of the possible explanations of negative practice data. In Part II the conclusions usually drawn from the negative practice data will be criticized on the following grounds: (1) The interpretation of *alpha*, *gamma*, and *beta* is ambiguous with respect to the precise meaning of repetition, so that it is difficult to evaluate the hypotheses until this meaning is clarified. (2) *Beta* in its usual form is not supported by disproof of *alpha* and *gamma*, when these hypotheses are interpreted in such a way that the effectiveness of negative practice is relevant evidence against them. (3) The negative practice experiments do not provide a situation in which all factors save the repetition of a stimulus-response sequence are held constant, and are, therefore, not crucial with respect to the role of any factor in learning. Part III will examine the adequacy of familiar theories of learning for explaining the negative practice results.

[4] In this case the authors are using Dunlap's 1932 definition of *beta* as the hypothesis that repetition decreases response probability.

II. Evaluation of Negative Practice Data as Evidence for the Beta Hypothesis

1.—Before embarking on a criticism of the conclusions which have been drawn from the facts of negative practice, it is necessary to consider the significance of the term, repetition, as it is used in the statements of the *alpha*, *gamma*, and *beta* hypotheses. There are a number of problems here which must be made explicit before it is possible to evaluate the negative practice data for disproving or supporting these hypotheses.

It may be asked, first of all, what processes or events that are important for learning must be involved in repetition; secondly, whether the effects of repetition are independent of the conditions in which the repetition occurs; and finally, whether it is possible to conceive of learning apart from the repetition of something. The last question may be answered briefly. Obviously some event must occur, some portion of a trial be carried out, some problem arise, some perception take place before behavior adjustment will be elicited. Even when insight is assumed to follow directly on the presentation of a problem, exercise will have been given in the single occurrence of the events preceding insight. This amounts to making the very obvious statement that some events must precede learning.[5]

The significant question is, therefore, not whether repetition is necessary but what must be repeated in order to produce learning and under what conditions practice must occur. Although, as pointed out, repetition may be given any of the processes in the repertoire of the organism, it is clear that the *alpha*, *gamma*, and *beta* hypotheses and frequency theories in general have referred specifically to the rehearsal of the stimulus-response sequence which is to be learned. Even so, there is need for more precise clarification of the meaning of repetition, for a stimulus and response may occur in a variety of conditions and the hypotheses under

[5] Tolman has discussed the meaning of repetition and would accept it as an important factor in this broad sense of the term (21, p. 347 ff.; 22, p. 378–387).

discussion might be interpreted to indicate the effectiveness of repetition under all these conditions or only under a limited number of them. It may well be asked, therefore, whether the *alpha* hypothesis means to imply that repetition of any stimulus-response in any and all circumstances produces learning of that sequence, or whether there are necessary limitations to this statement.

On first thought, the completely general statement of *alpha* that repetition of a stimulus-response sequence is always reinforcing seems to have been implied in many discussions of the frequency principle, particularly the earlier formulations of this point of view. Actually, however, nobody has ever really maintained that practice effects are independent of *all* conditions, for it has always been recognized that the behavior must be performed by an organism possessing a nervous system which is capable of retaining the effects of repetition. Furthermore, the fact that all versions of the frequency theory have made this and other inferences about the fundamental conditions of the effectiveness of repetition does not negate the importance of repetition, provided that it can be shown to be a real determinant of after-effects in the retentive nervous system thus assumed to exist. The fact does, however, constitute an important limiting condition of the effects of the factor in question.

This point is related to the criticism of the frequency principle on the grounds that repetition is a temporal factor which in itself cannot be thought of as a determinant of behavior (**10**, p. 561). Insofar as this criticism rests on the abstract nature of frequency, it can be refuted by the fact that no one need think of frequency as operating without reference to some concrete event. It is repetition of a particular reaction that is the alleged determinant of learning, not repetition in itself. If, on the other hand, the objection rests on the necessity for assuming the supporting role of intervening and mediating determinants which are not observed, then the immediately preceding discussion becomes relevant. *Any observed* determinant of behavior is presumably but one link in a causal chain of neuromuscular events,

or one factor which completes the field relationships necessary for the occurrence of a certain phenomenon, so that there is nothing unique in the necessity of inferring unobserved factors which condition the effectiveness of repetition.

Even if it be generally conceded that such inferred conditions are necessary for the rehearsal of a stimulus-response sequence to be an effective factor in learning, there are still other conditions, such as the effects of reward and punishment, which a theory about repetition may include or exclude as necessary for learning. The relation of these factors to the *alpha* hypothesis has not been discussed specifically, but it is clear that in some quarters the intention has been to imply by *alpha* that repetition of a stimulus-response sequence causes learning regardless of the presence, absence, or nature of the consequences. It is this version which the defenders of a law of effect would discard, while proponents of insight theories must reject this, as well as the more qualified contention that repetition of some stimulus-response-effect sequences results in learning while repetition of other sequences fails to do so.[6] Dunlap belongs in the latter group, for he emphasizes the point that the response practiced is not the response learned and so definitely rejects the importance of repetition of a particular stimulus-response sequence under any circumstances, in favor of what is essentially an insight theory of learning. For him thoughts, desires, and ideals are the important determinants of learning (**5**).

There is no intrinsic reason why one of these interpretations of *alpha* should be considered preferable to the other, but it should be noted that the negative practice results con-

[6] The question has been raised whether in this case it might not be possible to put in one category the effect and frequency theories which involve repetition of the stimulus-response sequence to be learned, leaving to another category all theories depending on assumptions about repetition of other processes which affect learning. The theories in the first category would be called frequency theories, the others, insight or expectancy theories. It cannot be said fairly, however, that the latter group involves only insight theories or that new principles must necessarily be evoked to explain learning in these cases. For example, learning by repetition of a stimulus and certain implicit responses which are functionally identified with the overt response to be learned may conceivably involve neither expectancy nor repetition of the overt response learned. Consequently, although insight theories deny the importance of repetition of the stimulus-response sequence, not all theories which rest on events other than frequency of this event can be called expectancy theories.

stitute an adequate test only of the more general form which considers repetition to be effective regardless of consequences, for the elimination of an error by negative practice and the strengthening of a correct response by positive practice is no disproof of a theory which holds that *some* stimulus-response-consequence sequences result in learning. The data leave such a statement unrefuted in spite of attempts like that of Dunlap to employ the negative practice results as evidence against *alpha* in both forms.

2.—Granting for the time being that the experiments in question were properly controlled, it may be concluded, therefore, that they constitute a disproof only of an *alpha* hypothesis which holds that repetition of a stimulus-response sequence always reinforces the probability of the occurrence of that response regardless of consequences. These same experiments also disprove *gamma* stated in similar fashion. As far as *beta* is concerned, however, it must be pointed out that disproof of *alpha* and *gamma* in this form does not lend any support to the usual implication given to *beta*, that repetition has no effect on learning under any circumstances, for this is not the logical alternative which follows when *alpha* and *gamma* are disproved. The statement of *beta* which does follow is that repetition of a stimulus-response sequence does not result in learning regardless of consequences, an interpretation which definitely fails to negate the effects of repetition in all conditions. Many conditions where repetition may be related to learning remain untested and unrefuted. It is unwarranted, therefore, to conclude, as many writers have done, that the negative practice experiments support the *beta* hypothesis. They are inconclusive on this point whatever interpretation is given *alpha* and *gamma*.

3.—Finally, even these limited conclusions which have been drawn from the experiments are conditional on the degree of experimental control maintained while securing the results, for *alpha* even in the general form, cannot be said to have been disproved by the demonstrated effectiveness of negative practice in weakening a wrong response, unless repetition of the stimulus-response sequence was the *only* varying

determinant of learning operative in the experiment.[7] If other factors were varying they may have entered to distort the true effects of repetition of the response observed. This is a familiar requirement of scientific procedure and since it is certain that no such condition was met by the experiments in question, disproof of *alpha* must be denied. The uncontrolled factors which may have operated to obscure positive effects of repetition will be discussed in Part III, section 1*a* and 1*b*.

It was pointed out in the introduction that the negative practice experiment had produced two major results: (1) demonstration of the effectiveness of negative practice in weakening the wrong response, and (2) the fact that the correct response was strengthened by a procedure which apparently involved no repetition of the correct response itself. Up to this time attention has been directed largely to the first result. It must now be suggested that the second is equally inconclusive with respect to the role of repetition in learning. The fact that learning seems to occur without repetition has sometimes been interpreted as a demonstration that repetition is not necessary for learning. It will be suggested in section III, however, that it is often possible that practice may have been given the correct response prior to the experiment and that the apparent strengthening of this response by negative practice is consequent to the weakening of the wrong response. If it is granted, however, that there are instances where responses seem to be strengthened without previous practice, it would then be necessary to admit that practice could not be a determinant of learning in those particular situations. Such a fact would indicate that there must be other determinants than repetition, though it would reveal exactly nothing about the role of repetition when and where it does occur.

[7] This statement is not intended to deny the validity of experiments using multiple variants, as advocated by Fisher (6) but the stated rule holds in these experiments as elsewhere, for if the role of a single factor is to be determined, only the values in one row or column of a latin square will be considered in order to discover the different values taken by the dependent variable when a single independent variable is being modified systematically and the other determinants are constant or varying in purely random fashion.

III. Theoretical Explanations of Negative Practice Results

Although the experiments under discussion do not provide crucial evidence for or against repetition as a determinant of learning, it is possible to ask whether the negative practice results may be accounted for by frequency theories in various forms or by any other consistent set of principles. No completely satisfactory explanation has been discovered but a number of critical points will be suggested in connection with several different theoretical systems as they are applied to the facts of negative practice.

It must be admitted in the first place that regardless of the theoretical structure employed in explaining the negative practice results, the situation clearly involves interference between antagonistic responses, for the correct response and the incorrect response cannot be performed simultaneously and one must be chosen to the temporary exclusion of the other. Under these circumstances the probability of the occurrence of any act is a function of its strength relative to the strength of other antagonistic acts which also tend to be called forth in a particular situation, and any factor which weakens one antagonist increases the probability of the appearance of the other without directly increasing its strength. If this is true, the primary theoretical task in hand is to enumerate the factors which may strengthen one act, the correct one, and those thay may weaken the other, the incorrect one, for either one or both processes must be involved in obtaining the reported results.

The following principles of response reinforcement and weakening will be discussed. (1) Repetition of a stimulus-response sequence without regard to consequences strengthens the response which has been repeated. This we have called the most general form of the *alpha* hypothesis. This principle will be applied to: (*a*) The conditioning of a response which is called forth by and practiced in the punishment situation and which inhibits the incorrect response. (*b*) Strengthening of the correct response by implicit positive practice. (2) Repetition of a stimulus-response sequence weakens a response

regardless of consequences. This is the *gamma* hypothesis. (3) Repetition of a stimulus-response sequence weakens the response which is followed by punishment and strengthens a sequence followed by reward. This is a more limited form of *alpha*, the law of effect. (4) The anticipation that a certain response is right or wrong is strengthened by repetition. When a demand to perform the correct act is present after these expectations have been built up, the correct act should follow and the incorrect act should be inhibited.[8]

1*a*. *Conditioned interference.*—If the *alpha* hypothesis in its general form is to be justified as a principle which can explain the apparent weakening of a response which is being repeated, the notion of conditioned interference must be invoked. Several writers have supported the theory that the disappearance of response with repetition results from the development of antagonistic reactions (**7, 23**) and in elaborating such a view Guthrie has assumed that the withdrawal of reward in extinction or the administration of punishment elicits a reaction which becomes conditioned to the stimuli which are present and thus interferes with the established responses to these same stimuli.

In order to adapt this theory to the situation under discussion, it would be necessary to hypothesize that knowledge of the incorrectness of a given reaction elicits a characteristic response of withdrawal or inhibition which is strengthened by practice and tends to interefere with the wrong response. To take a specific case of negative practice, let it be assumed that the typing error, *h-t-e*, is being repeated. It is extremely difficult to analyze out the instigating situations and responses, but at least the following would have to be considered: (1) the instructions, 'Spell *the*' (the correct spelling need not be represented explicitly or implicitly, as the instructions may be present as verbal processes), (2) the instructions, "Write *h-t-e* which is wrong," (3) a stimulus-act (some implicit verbal

[8] A principle of induction based on Pavlov's notion (**16**) that weakening of one response results in strengthening the other, not only relatively but absolutely, might be added to the list of effects of repetition. This is not discussed, however, since Pavlov has worked out the notion in connection with extinction rather than inhibition resulting from punishment.

response) representing the instruction, "Write *h-t-e*," followed by the overt response of writing these letters, (4) a stimulus act representing the instruction, "*h-t-e* is wrong" which has been assumed to be followed by a response of inhibitory character. After this complex of stimuli and responses has been repeated, the instruction, "Spell *the*," will tend to call out the implicit stimulus act which represents the instructions, "Write *h-t-e*," and this will in turn elicit inhibition of the writing of *h-t-e*, thereby releasing the tendency to write *t-h-e*.

Certain difficulties in this form of the theory are obvious. In the first place, nothing is known about the possible nature of the assumed inhibitory response. It is purely hypothetical and unspecific. Secondly, it is not clear just why such an inhibitory response should come to dominate the response of writing *h-t-e*, for according to the assumptions stated, there is no reason why all responses should not be reinforced equally, so that the response of writing *h-t-e* would be as likely to become conditioned to the stimulus act, "*h-t-e* is wrong," as for inhibition to appear in response to the stimulus act, "Write *h-t-e*." Other assumptions, particularly certain ones relating to gradients might be added to take care of this difficulty, except that the lack of evidence of temporal sequence in the events involved makes the application of such principles impossible. Similarly, Guthrie's idea that the last response to a stimulus is the one which will next occur does not seem to be appropriate, for again there is no reason to assume that the inhibitory response follows the response *h-t-e* during practice since both stimulus acts are presumably present throughout the experiment.

It is evident that an exact prediction of the relative effectiveness of positive and negative practice in establishing the correct response in this case would depend on quantification of the assumptions which the interference theory must make regarding the rate of increase in strength of the right, wrong, and inhibitory responses. In line with the theory it is assumed that positive practice involves no change in the strength of the wrong response, but only reinforces the correct one directly. On the other hand, in negative practice the

correct response remains at the level attained at the beginning of practice and the wrong one is strengthened by practice and weakened by interference of an inhibiting response called out by the punishment. The rate at which elimination of the error takes place will depend, then, on the rate at which repetition increases the strength of the error relative to the increase in the strength of the inhibitor.

1*b*. *Implicit positive practice.*—Another possible method of making the *alpha* hypothesis compatible with results involves adopting the suggestion that an individual in the negative practice situation may have engaged in implicit rehearsal of the correct response during his overt negative practice, so that the correct response was strengthened in this fashion while the wrong response was being strengthened simultaneously by overt repetition.

The first difficulty in explaining the results in this simple fashion lies in the question how practice of a surrogate response can strengthen the tendency to make the response itself, *i.e.*, how practicing *t-h-e* implicitly makes it possible to type *t-h-e* correctly. This is the problem of response equivalence for which there is as yet no definite explanation. It is also necessary to seek justification for assuming that such implicit processes occur and are effective in the particular situations in which negative practice has been shown to produce learning. None of the negative practice experiments has attempted to control or measure this variable and it can only be pointed out that observations have been made in other connections which indicate that implicit practice can result in learning. Perry (**18**) has recently reviewed this evidence and reported a new experiment in which 'imaginary' practice was shown to be effective in tasks ranging from tapping to digit substitution. Actually, however, these studies have not involved the performances and conditions of the negative practice experiments. Furthermore, even though implicit practice be assumed to be likely of occurrence in some cases, its existence must be doubtful in certain nonverbal situations where specific knowledge of the components of an act are difficult to identify and hence difficult to repre-

sent implicitly, as for example, in the muscular act of speaking. Other factors would have to be assumed to explain the reported success of negative practice in such a function.

It must also be remembered that if repetition is accepted as reinforcing regardless of consequences, then the wrong response is being strengthened at the same time that the implicit practice is strengthening the correct response. This brings up the question of how the correct act comes to be the stronger. Although it is conceivable that the effects of implicit positive practice might overbalance the strengthening of the wrong act by overt practice, the available evidence does not support the likelihood of such an assumption, for in general overt practice seems to be the more effective. Consequently, in spite of the fact that implicit practice doubtless occurs in many cases, it does not seem to be equal to the task of making the frequency principle wholly compatible with the facts.

2. *Repetition weakens.*—Such a theory as this, which is really Dunlap's *gamma* hypothesis, has not been elaborated by any writer as a general principle but brief attention will be given it since it constitutes one of the logical alternatives regarding repetition. If an attempt were made to explain experimental results in terms of this principle, it is obvious that conditioned interference could not be made to take care of the apparent inconsistencies as was done in the case of *alpha*. This means, then, that the disappearance of the wrong response could be explained by *gamma* but the success of positive practice would be difficult to handle. Furthermore, if a response were weakened on every occurrence, behavior would be even more unstable than it is, for an act could not occur many times before it would be superceded by another which had not been weakened by repetition. Moreover, this theory would necessitate assumptions about the origin of positive tendencies to action which must somehow be developed without practice, since practice is assumed only to weaken responses, although these responses already have sufficient potency to occur. A further obvious pitfall is the fact that if responses tend to lose their strength with every practice trial, a lifetime of action would be expected to ex-

haust completely the initial tendencies to action, leaving the organism as inert as a discharged battery. Indeed, the difficulties appear to be so numerous that further elaboration is unnecessary and the *gamma* hypothesis may be dismissed as an untenable alternative.

3. *Law of effect.*—Although a number of theorists lean heavily on some form of the law of effect, there is considerable disagreement on the details of this principle. The nature of the effects of punishment is a particularly controversial point, with Thorndike denying that punishment weakens and others criticizing the stand on factual grounds (**20**).[9] Mowrer has supported a modified law of effect which attributes no reinforcing influence to repetition alone, and assumes that reduction of tension following on a stimulus-response sequence is the essential factor in fixing that sequence (**15**). Furthermore, he, like Thorndike, denies that any new principle is at work in the observed effects of punishment and attempts to explain the disappearance of punished responses by the development of competing functions elicited by the punishment. Though this resembles Guthrie's explanation, it differs from it in holding that the competing inhibitory responses are established not alone by being repeated with a stimulus but by the fact that they are followed by release of tension.

To apply this specifically to the material under discussion, suppose that the situation, S_a, is conditioned to produce response, R_a, at the beginning of the experiment. This is the error response in the setting, "Perform the correct response." Then punishment, S_b (knowledge that R_a is wrong), is made to accompany S_a–R_a and elicits an inhibitory response, R_b. The question arises how S_a comes to call out R_b so that R_b occurs instead of R_a which disappears. Since, according to Mowrer, reinforcement follows only on tension reduction, such a factor must be found in order to explain the reinforcement of R_b. It is difficult to conceive the basis for the development and reduction of such tension in the usual negative practice situation. Inasmuch as the wrong response continues to be made for the duration of the experiment, there is no chance for the

[9] See Hilgard and Marquis for a discussion of the role of punishment (**8**, p. 125).

tension release which would be provided by absence of an error. Furthermore, whatever tension may be built up by the knowledge that the wrong response is being practiced is provided with no obvious occasion for reduction by the relief of knowing that the punishment has occurred and is over with, for no such climaxes of build up and release take place.

It is of course possible to assume that R_b is the implicit correct response which has already been discussed. In this case, the knowledge that R_a is wrong would be assumed to produce an implicit correct response, R_b. Here, again, some reinforcing agent for the sequence, S_a–R_b would have to be found. This might be the recognition that R_b is correct, though this possibility is not very convincing, inasmuch as this knowledge is present throughout the experiment and again the setting for building up and releasing tension seems to be lacking.

Since it is impossible as yet to agree upon the factors underlying the effects of punishment, it may be assumed for the remainder of the discussion that repetition of a stimulus-response sequence followed by punishment results in decrement in the punished response through some undetermined mechanism. Under these circumstances the probability of the occurrence of a correct response would be predictable if the following facts were available: (1) the rate of increase in strength of the correct response with increasing repetition under a given set of reward conditions; (2) the rate of decrease in strength of competing wrong response tendencies when repeated under a given set of punishment conditions; (3) the initial position of each antagonistic response tendency on its curve of reinforcement and on the curve for the weakening of that response, *i.e.*, the previous amount of reinforcement and weakening it has undergone before the practice under observation begins. Since the initial strength of a response will depend both on how much that response has been previously reinforced and how much weakened, and since the amount of change in strength with a given number of trials varies from trial to trial, it is necessary to know the number of reinforcing and weakening trials that have preceded the beginning of the

experiment in order to predict effects of future reinforcements and weakenings. If these facts could be known, it would be possible to predict how much each additional reinforcement trial would strengthen the correct response tendency, how much each trial under punishment would weaken the antagonist, and when the strength of one response would come to exceed that of its antagonist.

Although it is evidently impossible to determine the strength of responses at any given time independent of the observed frequencies with which they occur, it can be predicted on the basis of such an analysis that after a correct response has received extended practice and for some reason is still not dominant over the incorrect one, negative practice of the wrong response should be much more effective in establishing dominance of the antagonist than positive practice of that antagonist. This follows from the fact that further positive practice will not appreciably strengthen a response (in this case, the correct one) which is approaching its limit of reinforcement under the given conditions. In this event, and with motivating conditions unchanged, the response could be made more probable of occurrence *only* by weakening of the antagonist. This may well be the case with highly practiced functions such as the typing of very common words. The errors have persisted in spite of a high degree of practice in the correct response, and negative practice appears to be relatively more effective in such a situation. If, on the other hand, the competing response tendencies have certain other relative strengths, negative and positive practice would be expected to have equal effects on building up the correct response. This should be the case where the correct response tendency has not been reinforced to its limit and where punishment to the wrong response weakens it as much as reward reinforces the right response. To take still another case, it would be expected that negative practice would be totally ineffective only where the competing wrong response to the stimulus in question has attained no strength before the beginning of negative practice, and/or where the correct reaction has no potency. Since in the first case, where the wrong re-

sponse has no tendency to appear, negative practice would presumably be impossible and unnecessary, only the second case represents a possible situation. If, then, the positive response has no potency, mere removal of inhibition by weakening its antagonist cannot increase the frequency of appearance of the right response and negative practice will be ineffective. It is important that experiments be devised which will reveal the effect of such differences in initial strength of competing response systems on the relative effectivenes of negative and positive practice, for it is possible that such a factor may go a long way toward explaining the apparently contradictory results on the relative effects of negative and positive practice.[10]

4. *Expectancy theory.*—The suggestion has been made that effectiveness of negative practice is somehow connected with the fact that it enables an individual to gain voluntary control over his errors, so that he can choose the right instead of the wrong act (**21**). Such a description evidently implies concepts which are purposive in character and the question arises whether it must also demand the assumption of other explanatory principles than those already discussed. There is no quarrel regarding the necessity of recognizing the importance of incorporating the facts of purpose and expectation into a science of behavior and it is also generally accepted that the explanation of such facts will involve the postulation of unobserved processes which are inferred to intervene between

[10] It has been suggested that the present discussion is related to the experiments on retroactive inhibition which show that an increase in practice of an interpolated performance at first increases and then decreases the interference with a performance which has already been learned and is recalled following the interpolation. The most generally accepted theories of these results attribute the interference to a transfer of response tendencies from the interpolated responses to those learned in the original series. Melton and Irwin (**14**) have also proposed that some of the retroactive inhibition results from unlearning of the original responses when they appear during interpolated practice. This unlearning is transferred to the situation in which the original material is recalled. It is possible, of course, that some such factors of transfer and generalization need to be considered in negative practice and that practice of an interfering response may at an early stage of practice weaken and later strengthen its antagonist. However, the situations are so different that it is impossible to do more than suggest the possible parallel. The retroactive inhibition situation involves learning two different sets of responses successively and noting any resulting interference, while the negative practice set-up involves punishing verbally one response pattern of an antagonistic pair and noting its effect on the other member of the pair. These situations are not strictly analogous.

observed determinants and consequents. Hull, for example, finds it necessary to postulate stimulus traces, stimulus acts, excitatory tendencies and the like (**9**), while Tolman infers the presence of variables called expectations, demands, readinesses, and so on (**21**). The critical question is, however, not the naming of these factors but rather the method of inferring their presence and determining the relationship they bear to resulting behavior. In other words, it is necessary to know first of all what expectations or purposes are to be inferred as arising in a given situation, and secondly, what sort of behavior must follow when these purposes are operative. If the functional relationship assumed to exist between situation and expectation or between expectation and behavior is wrong, prediction of the behavior will be inaccurate.

Hull and many other theorists have made the frequency of occurrence of the stimulus-response-effect sequence, plus a number of subsidiary postulates, the basis of inferring his intervening processes, while Tolman, on the other hand, has denied the importance of a particular stimulus-response or stimulus-response-effect sequence. Tolman's principles have proven to be suggestive from a critical standpoint but because of their somewhat indefinite character it is difficult to subject them to the sort of evaluative process which has been applied to the principles of frequency and effect. There is no specific procedure, for example, by which to determine precisely the status of demands and purposes in the negative practice situation and in the absence of definite criteria it is possible only to attempt an inference on certain commonsense grounds regarding the determinants which might be judged to be operating and the behavior that should occur.

Such an attempt might begin, as already indicated, by stating that practice of an act with knowledge that it is wrong identifies the wrong act as such so that when it is desired to perform the correct act the practiced wrong one is voluntarily avoided and the correct one voluntarily performed. This statement implies that if the reactor knows which response is correct and if he desires to perform the right response, the correct response should follow thereupon, particularly if the

response is in the repertoire of the individual, *i.e.*, if he can perform it under some circumstances. As a matter of fact, all these conditions are met in the usual negative practice situations *before* practice takes place. The individual knows what the right response is. For example, he knows the correct order of letters in typing *t-h-e*, for he recognizes his mistakes. He desires to type correctly and he is capable of typing the letters in the proper order, for he does so on occasion, yet the correct act does not follow consistently in the presence of the desires and expectations thus inferred to be present. It is clear, therefore, that a purposive theory must account for this failure and indicate what purposive determinant is contributed by practice of the error which was lacking prior to practice. The point is related to Guthrie's criticism of Tolman's assumption that if a rat knows the behavior route to a desired goal, he will follow it (7).[11] The purposive theories in general offer no explanation of how expectation comes to dominate behavior but deal only with the question of how the expectation itself is built up. This treatment is strikingly inadequate in the present case.

There are doubtless possible ways of elaborating the theory in question to make it account for the results. It might be suggested, for example, that the apparent desire to write *t-h-e* correctly was in conflict with a hidden desire which prevented the correct action from taking place. The Freudian literature would, of course, contribute illustrations to support this interpretation. The fact remains, however, that there is as yet no definite means of inferring these conflicting purposes in the situation under discussion so that their evaluation is impossible in this connection. Furthermore, it is not apparent how the negative practice procedure would serve to remove such conflicting desires.

IV. Summary

It has been pointed out in Part II that the chief results of the negative practice experiments are the demonstration, (1) that repetition of a response may increase its strength, as in practice of the correct response in positive practice, or de-

[11] See Hilgard and Marquis (8, p. 89) for a discussion of this point.

crease it, as in the case of repetition of the wrong response in negative practice, and (2) that repetition of a wrong response with knowledge of its incorrectness may increase the probability of occurrence of the right response, which is then said to occur without repetition.

A review of the literature having to do with negative practice indicates that the most usual interpretation of these results has maintained that they confirm the *beta* hypothesis which states that repetition of a stimulus-response sequence never affects learning. A critical examination of the evidence has resulted in rejection of such a conclusion.

It is held that the first fact cannot be taken as confirmation of *beta* because: (1) the *alpha*, *gamma*, and *beta* hypotheses are so ambiguous with respect to the meaning of repetition that their evaluation must await the clarification of this term. The possible meanings are discussed. (2) If *alpha* is interpreted to mean that repetition of a stimulus-response sequence increases the probability that the stimulus will produce the response regardless of consequences, then *beta* as usually stated does not follow from a disproof of *alpha* and *gamma*. (3) Negative practice experiments do not provide a situation in which all factors save repetition of a stimulus-response sequence are held constant. In this case, the experimental data are not crucial with respect to the role of any factor.

In similar fashion, *beta* is unsupported by the second fact that learning seems to occur without repetition. It is pointed out that repetition of the correct response may have taken place prior to the experimental observation and furthermore, that even though learning should result without repetition in some cases, nothing is indicated thereby regarding the effects of repetition when it does occur.

Part III has discussed possible explanations of the observed effects of repeating a stimulus-response sequence with knowledge of its rightness or wrongness and has indicated certain advantages and disadvantages of the following principles: (1) repetition of a stimulus-response sequence strengthens the sequence regardless of the consequences of the response; (2) repetition weakens the response; (3) repetition of a stimulus-

response sequence reinforces the response when followed by reward and weakens it when followed by punishment; (4) the expectation that a certain response is right or wrong is strengthened by repetition. When a demand to perform the correct act is present along with such expectations the correct act should follow and the wrong one should be inhibited.

REFERENCES

1. Carr, H. C. *Psychology.* New York: Longmans, Green & Co., 1925.
2. Carroll, R. Analysis of the beta hypothesis. *School & Soc.*, 1929, **29**, 543–545.
3. Dunlap, K. A revision of the fundamental law of habit formation. *Science*, 1928, **67**, 360–362.
4. ——. Repetition in the breaking of habits. *Sci. Mo.*, 1930, **30**, 66–70.
5. ——. *Habits, their making and unmaking.* New York: Liveright, 1932.
6. Fisher, R. A. *The design of experiments.* London: Oliver & Boyd, 1935.
7. Guthrie, E. R. *The psychology of learning.* New York: Harper, 1935.
8. Hilgard, E. R., & Marquis, D. G. *Conditioning and learning.* New York: D. Appleton-Century Co., 1940.
9. Hull, C. L. Knowledge and purpose as habit mechanisms. Psychol. Rev., 1930, **37**, 511–525.
10. Hunter, W. S. Learning: II. Experimental studies of learning. In *Foundations of Experimental Psychology* (Ed., C. Murchison), Worcester: Clark Univ. Press, 1929.
11. Kellogg, W. N., & White, R. E. A maze test of Dunlap's theory of learning *J. comp. Psychol.*, 1935, **19**, 119–148.
12. Koch, H. C. The influence of mechanical guidance upon maze learning. *Psychol. Monogr.*, 1923, **33**, No. 147.
13. Ludgate, K. E. The effect of manual guidance upon maze learning. *Psychol. Monogr.*, 1923, **33**, No. 148.
14. Melton, A. W., & Irwin, J. M. The influence of interpolated learning on retroactive inhibition and the overt transfer of specific responses. *Amer. J. Psychol.*, 1940, **53**, 173–203.
15. Mowrer, O. H. Preparatory set (expectancy)—a determinant in motivation and learning. Psychol. Rev., 1938, **45**, 62–91.
16. Pavlov, I. P. *Conditioned reflexes.* (Trans. by G. V. Anrep), London: Oxford Press, 1927.
17. Peak, H., Brooks, J., & Hobson, B. Negative practice and correction of spelling errors. *J. Psychol.*, 1941, **11**, 103–114.
18. Perry, H. M. The relative efficiency of actual and 'imaginary' practice in five selected tasks. *Arch. Psychol.*, 1939, No. 243.
19. Poindexter, A. The factor of repetition in learning to type. *Kentucky Person. Bull.*, 1936, No. 17, 3–4.
20. Thorndike, E. L. *The fundamentals of learning.* New York: Teachers College, 1932.
21. Tolman, E. C. *Purposive behavior in animals and men.* New York: The Century Co., 1932.
22. ——. *Comparative psychology.* New York: Prentice-Hall, 1934, Ch. XII.
23. Wendt, G. R. An interpretation of inhibition of conditioned reflexes as competition between reaction systems. Psychol. Rev., 1936, **43**, 258–281.

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I. THE FRUSTRATION–AGGRESSION HYPOTHESIS [1]

BY NEAL E. MILLER (WITH THE COLLABORATION OF ROBERT R. SEARS, O. H. MOWRER, LEONARD W. DOOB AND JOHN DOLLARD)

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The frustration-aggression hypothesis is an attempt to state a relationship believed to be important in many different fields of research. It is intended to suggest to the student of human nature that when he sees aggression he should turn a suspicious eye on possibilities that the organism or group is confronted with frustration; and that when he views interference with individual or group habits, he should be on the look-out for, among other things, aggression. This hypothesis is induced from commonsense observation, from clinical case histories, from a few experimental investigations, from sociological studies and from the results of anthropological field work. The systematic formulation of this hypothesis enables one to call sharp attention to certain common characteristics in a number of observations from all of these historically distinct fields of knowledge and thus to take one modest first step toward the unification of these fields.

A number of tentative statements about the frustration-aggression hypothesis have recently been made by us in a book.[2] Unfortunately one of these statements, which was conspicuous because it appeared on the first page, was unclear and misleading as has been objectively demonstrated by the behavior of reviewers and other readers. In order to avoid any further confusion it seems advisable to rephrase this statement, changing it to one which conveys a truer impression of the authors' ideas. The objectionable phrase is the

[1] This and the following six articles are revisions of the papers read at the Symposium on Effects of Frustration at the meeting of the Eastern Psychological Association at Atlantic City, April 5, 1940. A. H. Maslow's, the Chairman's, paper was not read at the meeting because of lack of time.

[2] J. Dollard, L. W. Doob, N. E. Miller, O. H. Mowrer, and R. R. Sears. *Frustration and aggression.* New Haven: Yale University Press, 1939.

last half of the proposition: "that the occurrence of aggression always presupposes the existence of frustration and, contrariwise, that the existence of frustration always leads to some form of aggression."

The first half of this statement, the assertion that the occurrence of aggression always presupposes frustration, is in our opinion defensible and useful as a first approximation, or working hypothesis. The second half of the statement, namely, the assertion "that the existence of frustration always leads to some form of aggression" is unfortunate from two points of view. In the first place it suggests, though it by no means logically demands, that frustration has no consequences other than aggression. This suggestion seems to have been strong enough to override statements appearing later in the text which specifically rule out any such implication.[3] A second objection to the assertion in question is that it fails to distinguish between instigation to aggression and the actual occurrence of aggression. Thus it omits the possibility that other responses may be dominant and inhibit the occurrence of acts of aggression. In this respect it is *inconsistent* with later portions of the exposition which make a distinction between the instigation to a response and the actual presence of that response and state that punishment can inhibit the occurrence of acts of aggression.[4]

Both of these unfortunate aspects of the former statement may be avoided by the following rephrasing: Frustration produces instigations to a number of different types of response, one of which is an instigation to some form of aggression.

This rephrasing of the hypothesis states the assumption that was actually used throughout the main body of the text. Instigation to aggression may occupy any one of a number of positions in the hierarchy of instigations aroused by a specific situation which is frustrating. If the instigation

[3] *Op. cit.*, pp. 8–9, 19, 58, 101–102.

[4] *Ibid.*, pp. 32–38; also 27, 39–50, 75–87, 111, 166. In this later exposition a distinction is made not only between instigation to aggression and acts of aggression but also between conspicuous acts of overt aggression and inconspicuous acts of non-overt aggression. It is assumed that the former are more apt to be culturally inhibited by strong punishments than the latter.

to aggression is the strongest member of this hierarchy, then acts of aggression will be the first response to occur. If the instigations to other responses incompatible with aggression are stronger than the instigation to aggression, then these other responses will occur at first and prevent, at least temporarily, the occurrence of acts of aggression. This opens up two further possibilities. If these other responses lead to a reduction in the instigation to the originally frustrated response, then the strength of the instigation to aggression is also reduced so that acts of aggression may not occur at all in the situation in question. If, on the other hand, the first responses do not lead to a reduction in the original instigation, then the instigations to them will tend to become weakened through extinction so that the next most dominant responses, which may or may not be aggression, will tend to occur. From this analysis it follows that the more successive responses of non-aggression are extinguished by continued frustration, the greater is the probability that the instigation to aggression eventually will become dominant so that some response of aggression actually will occur. Whether or not the successive extinction of responses of non-aggression must inevitably lead to the dominance of the instigation to aggression depends, as was clearly stated in later pages of the book, upon quantitative assumptions beyond the scope of our present knowledge.[5, 6]

Frustration produces instigation to aggression but this is not the only type of instigation that it may produce. Responses incompatible with aggression may, if sufficiently instigated, prevent the actual occurrence of acts of aggression. In our society punishment of acts of aggression is a frequent source of instigation to acts incompatible with aggression.

When the occurrence of acts of aggression is prevented by more strongly instigated incompatible responses, how is the existence of instigation to aggression to be determined? If only the more direct and overt acts of aggression have been

[5] *Op. cit.*, p. 40.

[6] The notions used here are similar to those employed by Professor Hull in describing trial-and-error learning. See Hull, C. L. Simple trial-and-error learning—an empirical investigation. *J. comp. Psychol.*, 1939, 27, 233–258.

inhibited, as is apt to be the case because such acts are the most likely to be punished, then the instigation to aggression may be detected by observing either indirect or less overt acts of aggression. If even such acts of aggression are inhibited, then a different procedure must be employed. Two such procedures are at least theoretically possible. One is to reduce the competing instigations, such as fear of punishment, and observe whether or not acts of aggression then occur. The other is to confront the subject with an additional frustration which previous experiments have demonstrated would by itself be too weak to arouse an instigation strong enough to override the competing responses inhibiting the aggression in question. If the instigation from this additional frustration now results in an act of aggression, then it must have gained its strength to do so by summating with an already present but inhibited instigation to aggression. The presence of the originally inhibited instigation to aggression would be demonstrated by the effects of such summation. Thus the fact that an instigation may be inhibited does not eliminate all possibility of experimentally demonstrating its presence.

At this point two important and related qualifications of the hypothesis may be repeated for emphasis though they have already been stated in the book. It is not certain how early in the infancy of the individual the frustration-aggression hypothesis is applicable, and no assumptions are made as to whether the frustration-aggression relationship is of innate or of learned origin.

Now that an attempt has been made to clarify and to qualify the hypothesis, four of the chief lines of investigation which it suggests may be briefly considered.[7]

1. An attempt may be made to apply the hypothesis to the integration and elucidation of clinical and social data. Here the fact that certain forms of aggression are spectacu-

[7] Both of the first two of these chief lines of investigation have been developed at length in *Frustration and Aggression*. No attempt was made there to elaborate upon either the third or the fourth. Thus that first effort does not purport to be a complete systematization of all principles within a single field, but rather, an exploratory attempt to apply a strictly limited number of principles to several different fields. *Op. cit.*, pp. 18, 26.

larly dangerous to society and to the individual is relevant. This means that acute personality conflicts are apt to arise from the problem of handling aggression and that the problem of aggression is apt to play an important rôle in shaping certain great social institutions such as the in-group as an organization against the out-group.

2. An attempt may be made to formulate more exactly the laws determining the different ways in which instigation to aggression will be expressed under specified circumstances. Some of the problems in this field are suggested by the phenomena of displacement of the object of aggression, change in the form of aggression, and catharsis of aggression.

3. An attempt may be made to secure more information concerning the other consequences which frustration may produce in addition to the instigation to aggression. Such an attempt would lead into studies of rational thought and problem solution as suggested in the classical work of John Dewey, and into studies of experimental extinction, trial-and-error learning, substitute response and regression.[8] Work along this line of investigation may deal either with the clinical and social significance of these other consequences of frustration or with the discovery of the laws governing them.

4. An attempt may be made to improve or to reformulate the basic frustration-aggression hypothesis itself. The determination of the laws which allow one to predict exactly under which circumstances instigation to aggression may be expected to occupy the dominant, the second, the third, or some other position in the hierarchy of instigations aroused by a frustrating situation is a most important problem of this type. Another problem is the reduction of the frustration-aggression hypothesis to more fundamental principles and the more accurate restatement of the hypothesis in terms of these more basic principles. One of the steps in this direction would be to scrutinize any exceptions to the hypothesis as now formulated. Another step would involve a careful study of the early stages of the socialization of the individual in an

[8] These problems are discussed in more detail by Dr. Sears in the next paper of this series, 'Non-aggressive responses to frustration.'

attempt to analyze the interlocking rôles of three factors: first, innate physiological reaction patterns; second, learning mechanisms; and third, the structure of the social maze which poses the learning dilemmas and contains the rewards and punishments. An empirical and theoretical analysis along these lines might lead to a fundamental reformulation giving a closer approximation of the socially and scientifically useful truths imperfectly expressed in the present frustration-aggression hypothesis.

II. NON-AGGRESSIVE REACTIONS TO FRUSTRATION [1]

BY ROBERT R. SEARS

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In order to analyze the nature of the non-aggressive reactions to frustration it is necessary first to consider the systematic setting in which the concept of frustration lies. Any on-going activity derives from some kind of instigation. The activity itself represents a sequence of acts which are instrumental to putting the organism and its environment into such context with each other that the final consummatory act, or goal-response, can be made.

There are two possible consequences in this situation: First, the sequence of actions may be carried through to completion and the organism make its goal-response. In accordance with the law of effect, then, the instrumental act sequence is reinforced. Likewise the instigation to the performance of those acts and the goal-response itself is at least momentarily reduced. An alternative to this state of affairs is possible, however; the organism may not be able to make the necessary instrumental acts and there is interference with the occurrence of the final goal-response. This is frustration. As Miller (**6**) has emphasized, one (and *only* one) of the effects of this frustration is the production of a special secondary, frustration-induced instigation, the goal-response to which is aggression.

Such a statement says nothing about the effects on future behavior of the *primary* instigation, the goal-response to which has been frustrated. If one looks at this situation with reference to the instrumental acts and goal-responses concerned, it appears that there are at least three possible action sequences which may occur.

[1] Read at the 1940 meeting of the Eastern Psychological Association in the Symposium on Effects of Frustration.

1. The organism may continue or repeat the *same* instrumental acts leading to the *same* goal response. This is the kind of behavior G. V. Hamilton (4) described as being persistent and non-adjustive; it was somewhat more characteristic of lower than of higher animals and of children than of adults.

2. A *different* set of instrumental acts may be adopted to put the organism in position to perform the *same* goal-response. Thorndikian trial-and-error behavior appears to be largely of this kind, and various examples of what might be called instrumental act regression likewise represent simply a reactivation of earlier learned methods of reaching the goal. Children who are ill manifest this phenomenon quite frequently in their efforts to get attention and sympathy. Phantasy gratifications are occasionally of this order as well; in the Dembo problem subjects will frequently describe their imagined fantastic solutions with some defiance. If the experimenter tells them the proposed solution is absurd they may say, "Yes, but it would get the flower!"

3. A *different* set of instrumental acts may be instigated in order to put the organism in such position that it may perform a *different* goal-response from that which was originally frustrated. The Freudian concepts of regression and sublimation both represent this kind of reaction; regression to oral levels of gratification, for example, forces the adoption of different instrumental acts in order that the shift may be made from genital to oral goal-responses. But perhaps the most frequent example of this consequence of frustration is the so-called substitute response, in which the new goal-response that is performed has certain of the properties of the original frustrated goal-response and in some degree reduces the strength of the primary instigation (5).

The relation of these substitute responses to the phenomenon called by Lewin 'going out of the field' requires comment on another consequence of frustration, namely, its inhibitory or extinctive influence. Under some circumstances, the conditions of which have not yet been determined, the frustration appears to inhibit further action toward the orig-

inal goal-response and this inhibition generalizes to all the substitute responses. In such cases instigation to alternative responses unconnected with the original goal-response becomes relatively dominant and the organism either 'goes out of the field' or, if inhibition is complete, encysts itself. The degree to which these latter responses will occur appears to depend largely on the amount of generalized inhibition produced by frustration of the primary goal-response.

Examination of the clinical and theoretical literature will suggest many more varieties of response to frustration, of course, and, as anyone familiar with the work of Freud, Lewin, Murray and others is well aware, this systematic analysis of reactions to frustration in terms of instigation, instrumental acts and goal responses is not the only way in which they can be systematized. On the other hand, this analysis provides a behavioral and potentially experimentable basis for the understanding of such behavior.

From a research standpoint two problems become of immediate significance:

1. The exploratory problem of discovering the total repertory of frustration-reactions available to any individual. It is difficult to tell at present how much further the work in this direction must go before investigators can feel some confidence in the breadth of their perspective.

2. The determination of the specific factors which cause one kind of frustration-reaction rather than another to occur. Surprisingly few hypotheses have been suggested by psychoanalytic researchers to account for either individual differences in choice of reaction or the differences which occur from one occasion to another in the same person. Some little start has been made in this direction, however, by experimentalists. For example, Doob and Sears (**3**) have delimited certain of the conditions which differentiate between the occurrence of aggressive behavior and substitute responses; the factors which were shown to be of importance were the strength of instigation to the original (frustrated) goal response and the relative strength of anticipations of punishment for aggression. Dr. Helen Nowlis (**8**) has shown the influence of

reinforcement on the frequency of occurrence of substitute responses, and Brown (2) has determined certain of the conditions eventuating in 'going out of the field.' The work of Barker, Dembo and Lewin (1) and of Mowrer (7) on regression likewise contribute to this problem. Various other studies might equally well be cited as examples.

The determination of these various principles is of primary importance for any predictive science that aims at an incorporation of the phenomena of frustration in its systematic structure. Insofar as a theoretical system promotes rather than hinders such an effort, the system is useful. At the present stage of investigation the analysis in terms of instigation, instrumental acts and goal-responses appears to be of definite assistance. But only by continued use in conjunction with laboratory experimentation and in the interpretation and prediction of actual social or individual behavioral events can it be decided whether this is the *most* useful way of collating and conceptualizing the data.

REFERENCES

1. Barker, R. G., Dembo, T., & Lewin, K. *Studies in topological and vector psychology. No. 2. Experiments on regression and frustration.* Iowa City: Iowa Univ. Publication, new series, 1941.
2. Brown, J. S. Generalized approach and avoidance responses in relation to conflict behavior. Ph.D. Thesis, 1940, deposited in Yale Univ. Library.
3. Doob, L. W., & Sears, R. R. Factors determining substitute behavior and the overt expression of aggression. *J. abn. (soc.) Psychol.*, 1939, 34, 293–313.
4. Hamilton, G. V. *Objective psychopathology.* St. Louis: C. V. Mosby, 1925. Pp. 249–259.
5. Lewin, K. *A dynamic theory of personality.* New York: McGraw-Hill, 1935. Pp. 180–193.
6. Miller, N. E., Sears, R. R., Mowrer, O. H., Doob, L. W., & Dollard, J. The frustration-aggression hypothesis. Psychol. Rev., 1941, 48, 337–342.
7. Mowrer, O. H. An experimental analogue of 'regression' with incidental observations on 'reaction-formation.' *J. abn. (soc.) Psychol.*, 1940, 35, 56–87.
8. Nowlis, H. H. The influence of success and failure on the resumption of an interrupted task. Ph.D. Thesis, 1939, deposited in Yale Univ. Library.

III. NEED-PERSISTIVE AND EGO-DEFENSIVE REACTIONS TO FRUSTRATION AS DEMONSTRATED BY AN EXPERIMENT ON REPRESSION [1, 2]

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Reactions to frustration may be divided into two fundamental types. One of these looks to the fate of the frustrated segmental need, the other to the fate of the individual as a whole—namely, the integrated and coordinated personality—as a result of frustration. In other words, there is a type of reaction to frustration which serves to fulfill the frustrated need in spite of momentary obstructions; on the other hand are those reactions which serve to protect the integration of the personality if and when the latter is threatened by the frustrating situation. The former type of reaction may be designated 'need-persistive' and may be thought of as occurring invariably after frustration, whereas the latter, which may be designated 'ego-defensive,' is conceived as occurring only under special conditions of ego-threat. Most behavior incident to frustration entails both types of reaction but pure cases of each alone are found and the theoretical distinction thus appears justified as an aid to analysis.

The relationship of the present distinction to current psychoanalytic concepts is fairly obvious. One function of the present distinction is indeed to clarify the difference among psychoanalytic mechanisms and to assist in their experimental study. Thus, the concept of sublimation points chiefly to need-persistive reactions while that of projection concerns the defense of the ego. A fusion of need-persistive and ego-

[1] Read at the 1940 meeting of the Eastern Psychological Association in the Symposium on Effects of Frustration.

[2] A detailed account is to be published in the near future.

defensive reactions is found in the mechanism of reaction-formation since here an inhibited impulse gains indirect satisfaction (sublimation) while protecting the ego by a kind of displacement (projection). Most of the psychoanalytic mechanisms might be similarly allocated in the present framework.

A demonstration of the present distinction between need-persistive and ego-defensive reactions to frustration may be given by reporting an experiment designed to investigate the psychoanalytic concept of repression. Two groups of subjects were given a series of jig-saw picture-puzzles to solve. To one of the groups the puzzles were presented informally, for the ostensible purpose of helping the experimenter classify the problems for future use. The other group was given the same puzzles to solve but as an intelligence test. In both cases the subjects were permitted to finish half of the puzzles but were stopped midway in each of the remaining half. They were then asked to name the puzzles which they had attempted.

It will be readily appreciated that the subjects in the informal group were expected to be working under conditions of comparatively little tension whereas those in the formal group were being considerably aroused. While, in the one case, interest was mainly centered on the task so that incompletion could mean very little beyond residual tension related to the problem in hand, in the other case the pride of the participants had been definitely evoked and incompletion would almost inevitably be experienced as failure. In other words, in the informal group need-persistive reactions alone were presumably entailed while in the formal group ego-defensive ones were additionally brought into play. The hypothesis of the experiment was that under the informal conditions the unfinished tasks would be better recalled than the finished ones because need-persistive reactions alone would be operative and would make for the easier recall of tasks with which undischarged tension was associated. Conversely, subjects in the formal group were expected to recall finished tasks more frequently, the assumption being that with the arousal of pride and accompanying ego-defense in case of failure, the individual's needs for inviolacy would take

precedence over the task-tension making for the recall of the unfinished tasks.

The experimental results substantiated the hypothesis. In the group with informal conditions, 7 of the subjects recalled a preponderance of finished tasks and 19 a preponderance of unfinished tasks (4 showed no preponderant tendency), while in the group with formal conditions 17 recalled a preponderance of finished tasks and 8 a preponderance of unfinished tasks (5 showed no preponderant tendency).

It seems reasonable to conclude that while the present experimental results do not exhaust the possibilities of measuring the various aspects of need-persistence and ego-defense involved in repression, they at least demonstrate the basic distinction between the two reaction types. Theoretically this distinction proceeds from a psychobiological approach to the dynamics of behavior. It does not depend upon any special catalogues of drives or needs and hence falls outside the field of taxonomic controversy. It aids in the reformulation of psychoanalytic concepts along somewhat more precise and more experimental lines and, since it implicitly anticipates certain further differences in the reactions of individuals, it respects the uniqueness of the concrete personality.

IV. THE FRUSTRATION–AGGRESSION HYPOTHESIS AND CULTURE[1]

BY GREGORY BATESON

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I have been asked to examine the framework of *Frustration and Aggression*[2] from a specific point of view—that which comes from the experience of studying contrasting cultures—and for lack of space I shall confine myself rigidly to this point of view, at the risk of the reader assuming that I regard 'culture' as the answer to all our problems.

The greatest virtue of the book is that it is an attempt to *simplify* a great gamut of phenomena into a very condensed series of propositions. It attempts a formal picture, shaven as bare as possible with Occam's Razor. This simplification has this important virtue that while it makes the book easy to criticise, it also compels the critic to state his objections in terms of the simple formulations. Every such criticism must of necessity be constructive.

When we approach such a system of propositions, it is of no use to say something like this, "Your picture is in black and white, you make no mention of colour and I, the critic, am only interested in colour."

[1] Read at the 1940 meeting of the Eastern Psychological Association in the Symposium on Effects of Frustration. The fieldwork, on which this paper is based, was done by the author as a Research Fellow of St. John's College, Cambridge, 1936, and William Wyse Student in Social Anthropology, Cambridge, 1937–1939, in collaboration with Dr. Margaret Mead, of the American Museum of Natural History; with grants in aid from the National Committee for Mental Hygiene, through a grant from the Committee for Research in Dementia Præcox, founded by the Thirty-third Degree, Scottish Rite, Northern Masonic Jurisdiction, and the Social Science Research Council. For an account of the scope of this research see Researches in Bali, 1936–1939. *Trans. N. Y. Acad. Sci.*, Series II, 2, No. 1, 1939.

[2] J. Dollard, L. W. Doob, N. E. Miller, O. H. Mowrer, & R. R. Sears. Frustration and aggression. New Haven: Yale Univ. Press, 1939.

Rather, I think we should approach the formulations with the question "Can the sort of things which I, a student of culture, want to say, be said in terms of these given abstractions?" Let us, if we possibly can, avoid complicating the formulations, multiplying the entities beyond necessity.

It is true that the cultural matrix is not specifically mentioned in the basic definitions of the elements in the frustration-aggression sequence. The definitions are constructed almost as if the individual existed *in vacuo*. But, as a matter of fact, there are two places in the system in which culture, though not mentioned explicitly, is at least admitted by implication. Before we say therefore that the hypothesis makes no allowance for culture we must see how much latitude the formulations really allow us.

In the first place, culture is invoked by implication when the whole hypothesis is stated to refer specifically to observed *human* behaviour. This must necessarily mean 'cultural behaviour' since we know of no human behaviour which is not modified in terms of the social milieu in which the subject lives.

There are a few exceptions to this sweeping statement—spinal reflexes and intra-uterine reactions and perhaps some of the reactions of the new-born, but of these it may be said that we do not know enough about them to say that they are subject to cultural modification—nor can we at present apply the frustration-aggression hypothesis to these reactions. I think it very important that these reactions should be investigated—but for the present, at any rate, we do not know whether (for example) the temper tantrum of a new born baby is an instance of 'aggression' as operationally defined—a series of actions having as their reinforcing goal-reaction 'injury to some other organism or organism surrogate.' So, lacking more knowledge, we must leave aside these exceptions to the sweeping statement that all human behaviour is modified in relation to a cultural or social matrix, and assume that the frustration-aggression hypothesis refers simply to sequences of culturally modified acts.

The second point in the formulations, which implies that culture was in the minds of the authors, is the definition of aggression which I have just quoted—the reinforcement by injury to *some other organism or organism surrogate.* And in general it is assumed in later chapters of the book that the aggression will be directed in the first instance against the agent who did the frustrating.

Thus we see that the hypothesis is essentially a statement about series of *cultural behaviours in interpersonal contexts,* and it is evident that the authors regard it as such though they are willing to regard such behaviour as an animistic assault on a typewriter which will not work, a murderous daydream, or a lonely suicide, as extensions from their central theme.

Let me now try to take this central thesis into two strange cultures and briefly state whether the thesis can be made to fit. If we look at the Iatmul of New Guinea, we find that the thesis fits them perfectly, so that I need not waste much time on the details of their behaviour. The Iatmul when engaged in some series of activities which will bring a future satisfaction, will constantly look forward to that satisfaction as a means of diminishing the pains of present effort. And when they reach the satisfaction, they will heighten its value by looking back to the pains which went into the achievement. If they are interrupted, they will exhibit definitely aggressive behaviour. The thesis fits them, but they have added one wrinkle which is not provided for in the formulation—they have invested aggression with pleasure. For the Iatmul, aggression must be regarded as a self-rewarding action series, self-reinforcing regardless of whether it ends in injury to some other person.

And they go further than this—they habitually convert their conative efforts into imaginary aggression. The man who is cutting down a tree will excite himself to greater efforts by seeing himself as engaged in active assault upon the tree—or the child driving out the mosquitoes will smirch them with violent sexual abuse. On the whole, however, I think it would

be fair to say that the thesis fits the Iatmul at least as clearly as it does Europe.

But when we try to apply the same thesis in Bali, we get into difficulties, since it is hard to find a series of acts with a clearly defined reinforcing goal response. It is not that the Balinese behaviour disagrees with the thesis, but rather that the contexts in which we might look for the thesis can hardly be said to occur in Bali. The Iatmul and we ourselves see life as divided into sequences of neutral or unpleasant conative acts ending in satisfactions, but the Balinese do not see life like this. They are a busy, active people—but they are infinitely willing to suffer interruption. We never at any time saw a Balinese annoyed because he was interrupted in the course of some series of acts. They seem to take a very definite pleasure in mere activity in the present—in the very instant—either enjoying their own busy-ness or else ignoring what they are doing, letting their muscles run on automatically with the activity while their attention is given to some unreal world, singing the songs from the last opera which was performed in the village.

Now it is the assumption of the theory that the typical seriation of acts, punctuated by climaxes of satisfaction is basically human and ought to occur in all cultures, and therefore we must refer to the Balinese children.

We find that by and large the thesis can be applied to the children although it cannot clearly—at least not often—be applied to the adults. The children, however, can be frustrated and have temper tantrums when they are frustrated.

The problem remains as to how the children are modified so as to render them unfrustratable in these terms. The problem is something like this: "How is a certain structuralisation of sequences of acts taught to the child?" "How does it learn to see life as composed of smooth series of enjoyable acts rather than as separate sequences of acts where each sequence leads up to some satisfying climax?"

I can offer a partial answer to this question, but to state this answer I must modify the formulations of the frustration-

aggression hypothesis. The hypothesis invokes two types of switching from one sequence to another. The first type is called 'substitution' and here the reinforcing act is comparable to that of the original series. Substitution is defined in terms of partial satisfaction of the original instigators. The second type is the switch to aggression about which it is assumed that the reinforcing goal is fundamentally different from that of the original interrupted sequence.

In order to phrase the phenomena of Balinese conditioning, I shall have to assume that there is only one type of switching —that, in fact, the aggression sequence (the temper tantrum) is fundamentally only another case of substitute response.

So far, I have only simplified the formulations by reducing the number of entities, but I must add one complication. In order to bring the aggression sequence under the heading of 'substitution' I must assume that human acts are primarily and essentially inter-personal acts. I would say that the common element between eating the ice cream cone and hitting the mother is that both are events in a behaviour sequence involving the child and the mother. I would say that the receiving and perhaps the eating of the ice cream is for the child a pleasant small love climax in his relationship with his mother, while the temper tantrum is a hate climax in the same relationship. Either way, he gets his climax, and there is this much to be said for equating the two phenomena that among male primates and men we find a pretty strong tendency to confuse love making with aggression.

Now in the Balinese mother-child relationship, we find that the mother constantly enjoys titillating the child's emotions,—giving it a taste of behaviour sequences which the child might expect to end in climax. The mother enjoys the sequence but the climax does not occur. At the moment when the child either flings his arms around her neck or bursts into tears, the mother's attention seems to have wandered; she is in a brown study or she is speaking to somebody else.

In this way, I believe the Balinese child is driven not to expect or look for climax in his acts, but to take his pleasure where his mother took it—in preliminary steps with no de-

fined goal—and to live in the immediate present not in some distant goal.[3]

[3] After writing this paper, I had a very interesting conversation with Dr. Mittelmann about the rare contexts for rage among adult Balinese. I mentioned the following contexts: (1) At cockfights it is common to see something like rage expressed verbally against any infringement of the rules of procedure or against a man who bets and who confesses (when he has lost his bet) that he had no money. (2) A thief (especially a cattle rustler) is usually mobbed and killed if he is caught red-handed.

These contexts are interesting and I think that consideration of them will amplify the thesis of the paper. We may summarise that Balinese show rage whenever there is some sudden overt threat to the stability of that intricate series of conventions within which they live.

It is also interesting that cockfighting and gambling are, for the Balinese, in some sense *regressive* activities, to which men become addicted under stress of misfortune, and that cockfighting with its clearly marked climaxes of excitement between the cocks and among the spectators produces a psychological atmosphere a good deal different from that of daily life.

Among the Balinese, the common metaphor which is used to imply sexual attraction between boy and girl is "they stare at each other like two cocks with their hackle feathers up."

V. THE HOSTILE ACT[1]

BY DAVID M. LEVY
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In *Frustration and Aggression*[2] the situations in which frustration occurs always call for aggression, in terms of an attack on a frustrating agent. If no frustrating agent is present, then some object must be created for the purpose of relieving aggressive tendencies that arise in the frustrating situations. The need varies according to the tolerance to frustration. If the tolerance is poor, then some aggressor must be fabricated. A thwarted individual would then displace the aggression onto a system or onto any group representing a constellation of ideas that evoked hostility, no matter how mild, in the previous experience of the individual. This is all a familiar type of psychodynamics, namely the release of tensions arising from frustration and the use of an available object for its expression. To say, however, that aggression arises as a result of any frustrating experience is a generalization that requires scrutiny.

There are any number of frustrations that do not evoke aggressive response in the sense of discharging hostility against a social object or its surrogates. There are for example, a number of experiments in which animals are frustrated and in which such aggression does not occur. In my own work on the sucking behavior of dogs, an experiment was made in which two puppies were given adequate milk from the bottle, but fed so quickly that their sucking needs were never satisfied. In contrast to the two 'control' puppies who were given adequate satisfactions both of feeding and of sucking, the experimental animals showed no problem in aggression that could be directly traced to the sucking frustration. The result was rather a type of perverted sucking. They sucked each other,

[1] Read at the 1940 meeting of the Eastern Psychological Association in the Symposium on Effects of Aggression.

[2] J. Dollard, L. W. Doob, N. E. Miller, O. H. Mowrer & R. R. Sears. *Frustration and aggression.* New Haven: Yale Univ. Press, 1939.

their own paws, objects, and later on, after eating, they licked the plate interminably. In terms of general personality description, one of the experimental puppies could be described as more aggressive than the controls, the other less so. But these differences, it could be shown, were modes of reaction that occurred in the beginning of the experiment and were probably reinforced by the particular difficulty of the situation. I do not wish to elaborate this point, except to say that the puppy who was originally aggressive became more so, in certain respects, after the sucking difficulty was established, whereas the other puppy became more submissive.

In chickens in which pecking frustrations were produced, there was also no evidence of increased aggression. In the experiment 100 chicks were fed from troughs in the usual way, but prevented from pecking off the ground by covering it with a raised wire mesh. The control group of 100 in the adjoining half of the same chicken house were fed from troughs, but not prevented from pecking from the ground. The chickens on the wire pecked each other's feathers but, as the experiment revealed, the pecking was not due to increased aggression but to increased pecking needs. This same type of situation has been shown for other animals and for human infants also. The sucking frustrations in infancy cause finger sucking or sucking of other objects, as in the case of the experimental puppies, rather than increased aggression. There is no proof that the so-called weaning traumas of infancy cause more aggression or even more phantasies of hostility and the like than in other children. The same may be said of all those frustrations that have to do with bowel and bladder control.

A distinction may be made between the type of aggression described, especially in regard to sucking habits, and the situations of the type described by Dr. Dollard and his colleagues. The former may be called physiologic, the latter social types of frustration. However, Dr. Dollard has included the type of physiologic frustration I have mentioned as typically provocative of aggression. His generalization could be easily amended. It is a question, however, as to whether numerous situations in which the individual does not

deal directly with frustration readily translatable into terms of an aggressor, typically stimulate the aggressive rejoinder, for example, frustrations arising out of one's own inability to solve a mathematical problem and the like. The fact that tension may arise in any such instance, and that this tension is released by some motor action, whether tapping a pencil or pacing the floor, does not mean it is an aggressive act in a social sense. Furthermore, acts that typically call for aggressive behavior in certain individuals may affect others differently. To say that in such cases the aggression should follow but is repressed would require considerable study. Aggression is one of the ways of responding to frustration in a social situation. Presumably it varies in the strength of its impulse and its execution in different individuals. In the phantasies that occur during frustration, or in the choice of behavior to satisfy the particular tensions that arise, various possibilities occur. To state that only one possibility, namely the aggressive act, is the logical response to frustration, all others being forms of extemporizing, needs further proof.

The response of a child to the new baby is a very good example of a frustrating situation commonly evoking aggressive behavior in the form of an assault on the baby or mother, or both. However, though this pattern is seen most frequently, there are instances in which it does not occur. For example, a child may respond to the coming of the baby with, primarily, a desire to possess it, to have it for one's own. This is not an aggressive response in the form of an assault on a social object, yet the reaction may be a very strong one. The new baby may call forth a very strong maternal protective attitude, especially when the prior child is a girl, say, eight or ten years older than the new-born. In this situation there is a frustrating experience highly reduced as compared with one in which the age difference is only two or four years. Nevertheless, such situations may reveal frustration and yet show a maternal response as a primary determinant. It may be argued that the maternal response represents jealousy of the mother and hence a concealed aggression against her. That is to say, one may always argue in favor of the theory of aggression as against any other form of behavior in a

similar situation. As yet, we have no definite proof that that is always the case.

In regard to sibling rivalry, however, the aggressive response to the new baby is so typical that it is safe to say it is a common feature of family life. As seen in 'control situations' the aggressive act in its various forms is depicted so clearly that the dynamic process is worth describing. In the control situation dolls are used representing the baby at the mother's breast and an older child who stands for the patient. If it is a boy the question used to set off the behavior is: "And then the brother sees the new baby at the mother's breast. He never saw him before. What does he do?"

In reviewing the patterns of over 100 S–R experiments of children ranging from two to thirteen years, it is most useful to conceive of the act as an ongoing social process, a dynamic unit of behavior, with various influences brought to bear upon it in every phase. The completed primitive performance is an act in which the child attacks the baby doll and destroys it by biting it, tearing it with his fingers, or crushing it with his feet. If the experiment is repeated, there is, in most of the instances at ages three, four and five, a fulfillment of hostile activity of this type. In others there are varying approaches to this end-point, easily observed and measured. In the beginning of the act when, presumably, the impulse to attack is felt, one observes varying forms of inhibitions to the impulse, so that the act does not come out. These may be in the form of pauses, of saying, "I don't know what to do," of attempting to change the play into some other form, to play with other objects, or even to get out of the playroom. Sometimes the first act is to slap the doll standing for the subject. When a three-year-old girl was asked, "Why did you do that?" she said, "Because she was bad. She wanted to hit the baby." This type of response indicates that the hostile impulse had to be dealt with before the act occurred, that the impulse itself was judged to be bad, that the thought of the act had to be dealt with by punishment as though it were already fulfilled. The inhibition of the impulse may take the form of assumed stupidity like, "I don't know what you mean. I don't know what you want." Such inferences are

based on the fact that without any explanation, merely by saying "Go ahead" and repeating the experiment, the hostile act in such cases follows.

Without elaborating the meaning of the various kinds of inhibitions that take place before the act overtly occurs, it may be sufficient to say that we are dealing with repressions and, I believe, the equivalent of superego injunctions; that is to say, the child is in its impulse to act identifying itself with the attitude of a prohibiting parent.

Once the act goes into execution we see a number of efforts to deflect its aim so that the object of hostility will not be reached. At this point a common form of inhibition is a blocking of the act, as, for example, a slapping movement made at the doll which is held back. Another common form is displacement, whereby an object close to the doll is hit and the doll itself avoided. This may be seen also in the form of non-specific aggression, in which the child shoots at the ceiling, or at various objects in a direction entirely different from that of the baby doll.

So far we may say of the modifying influences that occur when the impulse to act is felt, the inhibiting influences tend to block the act when they occur at its initial point (the impulse) and, once the act goes into execution, to reduce it to a gesture or shunt it off.

Even when the object is directly hit, modifications of the attack at the target appear. For example, the hit may change into a touch. Instead of being struck at, the baby is just removed, or dropped. At this point, too, the attack may be disguised. The child takes the baby away and says, "It has to go to the hospital for an operation," and the like. As, presumably, the behavior of the child is released, the attack on the baby becomes free of all modifying forms and the object is destroyed in the manner described.

Even when the baby doll is attacked freely and crushed, the act is not necessarily completed. We are aware at this phase of a number of activities that prove its incompleteness. For example, a child after attacking the baby and crushing it, begins to defend itself for its behavior. The child says, "It was a bad baby." Another child says at this point, "We

don't need two babies in one house." Commonly at this stage the doll standing for the brother or sister is attacked, usually with the same method used on the baby. Another common pattern is seen in attempts to restore the baby, to make it come to life again, to fix it up, to deny what happened, and say, "The baby fell apart. Now it's all together again." One child at this stage said it was all a dream, it didn't happen. We see in these various performances that take place in the completion of the hostile attack on the baby, restoring behavior, self-retaliatory behavior, attempts at self-justification, and attempts to wash out the act by attributing to it the aspect of a dream or just play. In several cases children had a kind of war dance after achieving their purpose. In line with other patterns it would be interesting to speculate on this type of war dance as a way of warding off anxiety described in psychoanalytic literature.

Through these control situation experiments we see depicted the type of aggression of which Dr. Dollard and his colleagues have written in clear form, influenced in every stage of their study by the emotional problems of the individual.[3]

[3] Certain applications of the dynamic pattern presented are being made to a number of problems in psychodynamics. In various anxiety symptoms, for example the phobias, we see inhibitions to the act at the impulse. In the tics of children we see inhibitions to the act while it is taking place, for example, tics of the eyelid representing inhibition of sex curiosity, mouth-opening tics representing an inhibition of biting tendencies, certain choreic-like tics of the hand representing incompleted masturbatory behavior. I think the application of this concept would solve a problem presented in Freud's *The Problem of Anxiety*. In Chapter IV of that book he deals with his correction of a theory of anxiety, to the effect that anxiety does not emanate from repressed libido, but from the attitude of the ego. Anxiety is caused by the ego's awareness of danger. This danger may come from without or from instinctive impulses within the organism, that is to say, from libido. Freud states that anxiety is not due to transformation of libidinous charges as he thought formerly, but the ego's awareness of the danger of these charges. The repression that follows is due, therefore, to the activity of the ego, not to a transformation of libido into anxiety which is then later repressed. However, Freud found his old theory quite adequate in its application to anxiety arising in sexual behavior, in which the sexual act is not completed in a true physiologic sense. In the latter instance he adhered to the view that the undischarged libido is converted into anxiety, recognizing a contradiction of the two viewpoints. The contradiction is solved if we consider the phase of the act from which the anxiety emanates. In the first set of circumstances, the phobias, it emanates when the impulse to act occurs. In the situation of frustrated sexual behavior the inhibition occurs at the close of the act or, at least, when the act has gone to a near degree of completion. Hence, anxiety may arise during the act at any phase, and the rule that it arises from the inhibition of the act at any given point may still be maintained.

VI. FRUSTRATION PHENOMENA IN THE SOCIAL AND POLITICAL SPHERE

BY GEORGE W. HARTMANN

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The 'thwarted' experience or condition resulting from the failure of an organism to reach some valued objective is a familiar aspect of everyday life. Nonetheless, surprisingly little consideration has been given to the manner in which different cultures make provisions for accommodating themselves to the post-frustration behavior of 'balked' individuals or for adjusting these persons to their own unsatisfying state.

A voter who casts his ballot for a given candidate or proposal is normally probably disappointed when the persons or programs he endorses are not supported by enough other voters to become effective in the life of the community. In America, we have established the tradition of the 'good loser,' *i.e.*, the defeated groups accommodate themselves with as good grace as they can to the victory of their opponents. Additional comfort is derived from such maxims as "Every dog has his day" and "Lose a battle to win a war." In a pluralistic society such as we have in the United States, defeat on the political front is not as decisive or as all-inclusive in its effects as a similar occurrence in totalitarian cultures where the loss of the political struggle approximates a loss of all one's basic values. In other words, the contemporary American pattern of political frustration involves a partial but not a complete thwarting; similarly, the successful partisan enjoys a more limited triumph because less has been staked upon the outcome. The gap between winner and loser is plainly narrower than in the dictatorial states, where an all-or-none folkway rules.

The candidate for public or organizational office is in much the same psychological situation as the voter, save that his responses are more focalized and intense. How he will

take his rejection by the electorate is a function of many things, but notably of those aspects of personality organization loosely labeled 'temperament.' If he is a minority party candidate, his frustration is not particularly acute because he knows in advance that he is not likely to win; however, a decline in balloting strength over that earlier exhibited by his group does have a disconcerting effect. He may become so angry and discouraged as never to run again, or he may study his 'failure' in order to discover the principles of successful tactics. It is hard to arouse much public sympathy for a program of mental hygiene for losers at the polls, largely because most candidates are extroverts who can take a licking and whose place in the community is such that compensatory satisfactions in other fields are readily available to them. However, high-level frustration which results in denying to the community the services of talented persons who have been impelled to go out of their field is a phenomenon that cannot be viewed with indifference.

Another acute type of frustration, commonly neglected in favor of more obvious forms, is the frustration which a sensitive 'reformer' experiences in the presence of the 'needless' frustrations that others undergo. Some persons are so constituted that they are profoundly distressed by their awareness of the thwartings of their fellows. This may be a secondary or higher-order category of frustration. If, in addition, one's efforts to help another out of the frustrating state of affairs in which he finds himself are rebuffed, something like a tertiary order of frustration is approached.

Most attempts at social reconstruction may be viewed as efforts to reduce the amount of frustration in group life. Some may increase it while attempting to do the contrary! Techniques for minimizing, if not eliminating, thwarting are very few. Progressive education at its best seems to be one successful procedure; the personnel movement in industry may be another. Democracy, cooperative socialism, pacifism, etc., are all devices which may be tested empirically for their value in decreasing the frustration of contemporary man by re-arranging the field that embraces him.

VII. DEPRIVATION, THREAT AND FRUSTRATION[1]

BY A. H. MASLOW

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It is easy in the discussion of frustration to fall into the error of segmenting the human being. That is to say, there is still a tendency to speak of the mouth or stomach being frustrated, or of a need being frustrated. We must keep in mind constantly the truism that only a whole human being is frustrated, never a part of a human being.

With this in mind, an important distinction becomes apparent, namely the difference between deprivation and threat to the personality. The usual definitions of frustration are in terms simply of not getting what one desires, of interference with a wish, or with a gratification. Such a definition fails to make the distinction between a deprivation which is unimportant to the organism (easily substituted for, with few serious after-effects), and, on the other hand, a deprivation which is at the same time, a threat to the personality, that is, to the life goals of the individual, to his defensive system, to his self-esteem or to his feeling of security. It is our contention that only a threatening deprivation has the multitude of effects (usually undesirable) which are commonly attributed to frustration in general.

A goal object may have two meanings for the individual. First it has its intrinsic meaning, and secondly, it may have also a secondary, symbolic value. Thus a certain child deprived of an ice-cream cone which he wanted may have lost simply an ice-cream cone. A second child, however, deprived of an ice-cream cone, may have lost not only a sensory gratification, but may also feel deprived of the love of his mother because she refused to buy it for him. For the second boy the ice-cream cone not only has an intrinsic value, but may

[1] Prepared for the 1940 meeting of the Eastern Psychological Association in the Symposium on Effects of Frustration.

also be the carrier of psychological values. Being deprived merely of ice-cream *qua* ice-cream probably means very little for a healthy individual, and it is questionable whether it should even be called by the same name, *i.e.*, frustration, which characterizes other more threatening deprivations. It is only when a goal object represents love, prestige, respect, or achievement that being deprived of it will have the bad effects ordinarily attributed to frustration in general.

It is possible to demonstrate very clearly this twofold meaning of an object in certain groups of animals and in certain situations. For instance, it has been shown that when two monkeys are in a dominance-subordination relationship a piece of food is (1) an appeaser of hunger and also (2) a symbol of dominance status. Thus if the subordinate animal attempts to pick up food, he will at once be attacked by the dominant animal. If, however, he can deprive the food of its symbolic dominance value, then his dominator allows him to eat it. This he can do very easily by a gesture of obeisance, *i.e.*, presentation as he approaches the food; this is as if to say, "I want this food only to still hunger, I do not want to challenge your dominance. I readily concede your dominance." In the same way we may take a criticism from a friend in two different ways. Ordinarily the average person will respond by feeling attacked and threatened (which is fair enough because so frequently criticism is an attack). He therefore bristles and becomes angry in response. But if he is assured that this criticism is not an attack or a rejection of himself, he will then not only listen to the criticism, but possibly even be grateful for it. Thus, if he has already had thousands of proofs that his friend loves him and respects him, the criticism represents only criticism; it does not also represent an attack or threat.

Neglect of this distinction has created a great deal of unnecessary turmoil in psychoanalytic circles. An ever-recurring question is: Does sexual deprivation inevitably give rise to all or any of the many effects of frustration, *e.g.*, aggression, sublimation, etc. It is now well known that many cases are found in which celibacy has no psychopathological

effects. In many other cases, however, it has many bad effects. What factor determines which shall be the result? Clinical work with non-neurotic people gives the clear answer that sexual deprivation becomes pathogenic in a severe sense only when it is felt by the individual to represent rejection by the opposite sex, inferiority, lack of worth, lack of respect, or isolation. Sexual deprivation can be borne with relative ease by individuals for whom it has no such implications. (Of course, there will probably be what Rosenzweig calls need-persistive reactions, but these are not necessarily pathological.)

The unavoidable deprivations in childhood are also ordinarily thought of as frustrating. Weaning, elimination control, learning to walk, in fact every new level of adjustment, is conceived to be achieved by forceable pushing of the child. Here, too, the differentiation between mere deprivation and threat to the personality enjoins caution upon us. Observations of children who are completely assured of the love and respect of their parents have shown that deprivations can sometimes be borne with astonishing ease. There are few frustration effects if these deprivations are not conceived by the child to be threatening to his fundamental personality, to his main life goals, or needs.

From this point of view, it follows that the phenomenon of threatening frustration is closely allied to other threat situations much more than it is to mere deprivation. The classic effects of frustration are also found frequently to be a consequence of other types of threat—traumatization, conflict, rejection, severe illness, actual physical threat, imminence of death, humiliation, isolation, or loss of prestige.

This leads us to our final hypothesis, that perhaps frustration as a single concept is less useful than the two concepts which cross-cut it, (1) deprivation, and (2) threat to the personality. Deprivation implies much less than is ordinarily implied by the concept of frustration; threat implies much more.